

1766
E S S A Y S

AND

OBSERVATIONS,

PHYSICAL AND LITERARY.

*Read before a Society in EDINBURGH,
and published by them.*

VOLUME I.

EDINBURGH:

Printed by G. HAMILTON and J. BALFOUR,
Printers to the University.

M,DCC,LIV.

22

ESSAYS

AND

OBSERVATIONS

PHYSICAL AND LITERARY

Jos: Banks

EDINBURGH:

Printed by G. Hamilton and J. Hamilton,
Printers to the University.

MDCCLXXV.

P R E F A C E.

AFTER the medical society of *Edinburgh* had published those volumes of *Essays*, which have met with so favourable a reception from the public, a proposal was made them to enlarge their plan, and to carry their disquisitions into other parts of nature, beside such as more immediately relate to the branches of medicine. All the sciences are remarked to have a close connexion together ; but none more than those of medicine and natural philosophy : And the society soon observed, that, should it turn its inquiries into more general knowledge, it could reap the advantage of preserving all its old members, and needed but open its door to Gentlemen of other professions, who might enrich it with their observations and discoveries.

Soon after the society had received a new form, several misfortunes happened, which retarded its progress, and have hitherto prevented it from communicating any thing to the

the public. The rebellion broke out in this country; and both scattered the members for some time, and engaged their attention to subjects, less agreeable and more interesting, than general disquisitions into nature. No sooner were public affairs composed, than we met with an irreparable loss in the death of Mr. MACLAURIN, one of our secretaries. The great talents of that Gentleman are generally known and highly esteemed in the literary world; but the society have, also, particular reason to regret in him the loss of those qualities, which form an excellent academician. Indefatigable himself, he was a perpetual spur to the industry of others; and was highly pleased with the promotion of knowledge, from whatever hands it came. At the time of his death, a number of discoveries, sufficient to have formed a volume, had been communicated to him; but, being mingled with his other papers, have been dissipated by various accidents; and the society could recover but few of them.

THE

P R E F A C E.

v.

THE object of this society is the same with that of the other academies, which have been established in different parts of *Europe*, the promoting of *natural philosophy*, and of *literature*, by communicating to the public such dissertations as shall be transmitted to them, either by their own members or by others. 'Tis allowed, that these two branches of learning, especially the former, are more promoted by the observation of facts than by the most ingenious reasonings and disputations. To a diligent, and even sometimes to a careless inquirer, many valuable experiments, no doubt, occur; and these would enrich our collections, tho' without this method of conveyance, they would be intirely lost to the public. The united judgments too of men correct and confirm each other by communication, their frequent intercourse excites emulation, and from the comparison of different *phaenomena*, remarked by different persons, there often result general truths, of which, from one of these *phaenomena*, no man of the greatest sagacity could entertain
any

any suspicion. Tho' the collection of experiments seems continually, by means of the learned societies, and the labours of individuals, to be augmenting, we need not entertain any apprehensions, that the world will ever be overwhelmed by the number of confused and independent observations. The heap does not always go on, increasing in bulk and disorder, thro' every age. There arise, from time to time, bold and happy geniuses, who introduce method and simplicity into particular branches of science; and reducing the scattered experiments to more general theorems, abridge the science of nature. Hints of this kind, we hope, may be able to pass thro' our hands; and at worst, our collections will be a species of magazine, in which facts and observations, the sole means of true induction, will be deposited for the purposes of philosophy.

THE sciences of theology, morals, and politics, the society are resolved intirely to exclude from their plan. However difficult the inferences in these sciences, the facts, on
which

which they are founded, are extremely obvious ; and we could not hope, by our collections, to be, in this respect, of any service to the public. The great delicacy of the subject, the imperfections of human understanding, the various attachments and inclinations of mankind, will for ever propagate disputes with regard to these parts of erudition. And 'tis the peculiar happiness of geometry and physics, that, as they interest less the passions of men, they admit of more calm disquisition and inquiry.

It is not that the society expect or propose, that what they communicate will be intirely above doubt or disputation. The papers, indeed, which they print, were all read before them, and they gave their consent to the inserting them in their collections: But they pretend not to warrant the justness of every reasoning, nor the accuracy of every observation. The author alone of each paper is answerable for the contents of it: And the society are as willing to insert what may be communicated in opposition to the sentiments

ments of any of its members, as in confirmation of them.

THE society are sensible that it belongs alone to the public to decide concerning the value of any invention; and all the merit to which they pretend, is that of exciting the industry of the learned, and of conveying their productions to the notice of the world. They assume not such authority as to stamp their approbation on any performance, even those which they communicate, much less those which have barely been read before them. If ever their testimony has been cited by any author in favour of his books, reasonings, or machines, they hereby declare, that this liberty has been used entirely without their consent or knowledge, and proceeded alone from the fond opinion, which the writer had entertained of his own performance.

WHOEVER will favour the society with any discourse which it comprehends in its plan, may send their papers to either of the secretaries, Mr. ALEXANDER MONRO Professor of Anatomy at *Edinburgh*, or Mr. DAVID HUME Library-keeper to the faculty of Advocates.

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ESSAYS

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ARTICLE I.

*Of the Laws of Motion; by the Honourable
HENRY HOME, Esquire, one of the Senators
of the College of Justice.*

Nothing has more perplexed philosophy, than an unlucky propensity, which makes us grasp at principles, without due regard to facts and experiments. Tho' fond of knowledge, we are willing to purchase it at the easiest rate; and general principles delight us, because they shorten the road to knowledge. This bent of the mind is productive of manifold errors. Prepossessed once by a favourite principle, we are no longer open to conviction. Every phe-

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nomenon

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nomenon must be accommodated to that principle, and every opposite fact, however obstinate, must go for nothing. And thus we endeavour to mould nature to our wish, instead of desiring to know nature in her genuine figure.

WE see, then, that in science, as well as in action, appetite and inclination generally gain the ascendant. Even in natural philosophy, theory was introduced before experiment, and every philosopher urged his own notions, without regard to truth or reality. This produced a mass of undigested and contradictory theory; which at length could not fail to bring on the discovery, that the whole was little better than fancy and chimera. The discovery had a remarkable good effect; which was, first to make us doubt of every thing, and then to make us search after truth in the more painful road of induction. By this means, a greater number of important truths have been brought to light within a century or two, than before that time from the beginning of the world.

BUT tho' our only sure guides to truth are facts and experiments, it is however expedient to keep the end in view. Facts and experiments

periments are useless lumber, if we are not to reason about them, nor draw any consequences from them. In all our operations, we may have an eye to theory: nay we must have it; for such is the constitution of our mind. Theory becomes only a source of error, when we indulge in it too much, or attach ourselves to it beyond what facts and experiments can justify. In short, theory is vain without experiments, and experiments are best understood by applying them to theory.

NATURAL philosophy, it must be acknowledged, is far from being ripe for firm and lasting theory. But we have facts and experiments in great abundance, to be a foundation for theoretical sketches; which may be safely indulged, provided they be done in so slight a manner, as to leave the mind free from any wrong bias. Tho' we have not materials for a regular building, there is nothing to hinder us to venture upon a model.

ALMOST every author who treats of the laws of motion, has thought it incumbent upon him to set out with a definition of motion. And, in a thing so simple, it may appear strange to find such a variety of definitions: whence one thing may be gathered, that none

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of those definitions have been approved of by succeeding authors. *Epicurus* defines motion to be "a passage from one place to another:" and he might as well have defined it to be motion from one place to another; because passage in this sentence means nothing different from motion. *Sturmeus* defines motion to be "*successiva rei motæ in diversis locis existentia*;" which is justly blamed by *Leibnitz* as describing the effect of motion, rather than the formal nature of it. And yet *Leibnitz*'s own definition, "*Motus est continua loci mutatio*," is not more satisfactory. But, of all definitions that ever were attempted, *Aristotle*'s definition of motion is the most unintelligible, "*Actus entis in potentia quatenus in potentia*," which *Locke* condemns as absolute jargon; and which, he says, would puzzle any rational man, to whom it was not already known by its famous absurdity, to guess what word it could ever be supposed to be the explication of.*

BUT it is no wonder these authors have not given satisfaction to each other, or to their readers, upon this subject; for they have attempted what is utterly impossible,
viz.

* *LOCKE* concerning human understanding, B. 3. ch. 4. § 8.

viz. to give a definition of a simple idea. They might as well have attempted a definition of colour or sound, of pain or any other simple feeling. It is to be regretted, that natural philosophers and mathematicians are not always well skilled in logics, and of this truth we shall have but too often occasion to give examples.

SCARCE any author that I know has thought of giving a definition of rest; yet it is not a more simple object than motion. I can guess at the reason, that rest is the more familiar object; a greater number of things appearing to be in a quiescent state than in motion. If every thing about us appeared to move, we should be surprised the first time we saw a body at rest, and probably would endeavour to explain the thing to others: but both are equally incapable of a definition. They are simple objects of sight and perhaps of touch. Deprive us of these two senses, and we could never have the least notion either of rest or motion,

WHEN a being moves itself or moves, we conceive the being as acting; and in this view motion is a species of action. But, when a body is moved by being acted upon,

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we conceive the motion of the body as an effect produced by a proper cause. In this case the body does not act, but is acted upon. This, tho' an evident distinction, does not exclude self-motion from being also considered as an effect in a certain light, *viz.* an effect produced by a being upon itself.

EVERY thing which moves, and is not barely moved or acted upon, must be endued with a power of motion. For this power is involved in the very conception of self-motion. And the term *power* may be equally applied to animate and inanimate beings, supposing them to be equally self-movers.

WHETHER matter in any case be endued with a power of motion, is a celebrated question, which is generally resolved in the negative: and, as this question cannot be overlooked in the present speculation, I shall endeavour to examine it with all the accuracy possible.

MATTER is generally represented as altogether unactive and inert; and indeed in a superficial view the fact appears to be so. The bulk of the things about us seem to be at rest, and we lay our account that they will continue in that state, unless acted upon
by

by some external force. If we lock up any moveable object, we trust to find it in the same place; and, if it be missing, we ascribe our loss to thieves, not to self-motion in the body. Matter, so far as we can discover, is certainly not endued with thought or voluntary motion; and yet, that it is endued with a power of motion in certain circumstances, appears to me an extreme clear point. Dropping a stone from a high tower, it falls to the ground without any external impulse, so far as we can discover. Here is an effect produced, which every one, who has not studied philosophy, will attribute to a *power* in the stone itself. One would not hesitate to draw this conclusion, should the stone move upwards; and yet, setting aside habit and custom, it must be evident, that a stone can as little move downwards as upwards without a *vis motrix*. And that this is a just, as well as natural way of thinking, will appear by analogy. When a man is in motion, we readily ascribe the effect produced to a power, which he possesses, to move his limbs. Why then do philosophers deny to the stone, in the act of falling, the power of beginning motion, a power which they so readily ascribe to the man?

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man? If it be objected that man is a being endued with a power of moving himself, and of moving other things, the plain answer is, that these are facts which we learn no other way than by experience; and we have the same experience for a voucher, that a stone set free in the air will move itself. And if it be farther urged, that man is a thinking being, the answer will readily occur, that a power of beginning visible motion is no more connected with a power of thinking, than it is with any other property of matter or spirit. Nay, Mr *Locke* holds, that matter may be endued with a power of thinking; and, supposing this power super-added to the other properties of matter, it cannot be maintained, that matter would be rendered thereby more or less capable of beginning or continuing visible motion.

BUT this is not the only instance in which we discover an agency or active power in matter. A billiard ball struck upon the ground, rebounds with a considerable force. A bow bent by the hand, restores itself with violence when the string is let go. In both, there is an instant of rest betwixt the opposite directions of the motion. The ball rebounds, and the bow
restores

restores itself to its former shape, without any external force, by an inherent power which is known by the name of elasticity. But we need not dwell upon particular instances. Chemistry discovers various powers in matter of the most active kind; and every man who is conversant with the operations of chemistry, must have a strong impression that matter is extremely active.

For the sake of illustration, let us suppose a substance or thing having the following properties, that it makes no resistance to bodies impinging upon it; that when carried along in a man's hand, even with the most violent motion, it does not increase the *momentum* of the hand, and that it stops short the instant the hand ceases to move. This would certainly be the most inert of all conceivable things. And if so, matter cannot be absolutely inert or passive, when its properties differ so widely from these described. In many circumstances matter begins motion, and acts often with great violence. It has a constant endeavour, when once set in motion, to preserve itself in the same degree of motion, and, when at rest, is not put in motion without resistance.

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BUT

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BUT it is maintained by the bulk of our philosophers, that matter is altogether incapable of active powers; that activity is confined to immaterial substances, and that inertness is implied in the very conception of matter. This moves them to ascribe to some invisible agency, all that activity which we discover in matter. In every one of the above instances, matter, they say, does not act, but is acted upon by the deity, who interposes by general laws, to preserve the uniformity of nature. Thus, when a stone falls, it is not the stone which acts, but the deity. It is the continual action of the deity which keeps the planets in elliptical orbits: and, when a plague infects the world, it is the deity who spreads the infection, and directs inert matter to ravage and destroy. Arsenic is not of itself a poison; it is the immediate finger of God which makes it so.

WITH regard to this whimsical doctrine, which declares war against our senses, it may be observed, that natural philosophy is not much affected by it, of whatever errors it may be productive in the more abstract sciences. For it is admitted, that the deity, in acting upon matter, subjects himself to the same
strict

strict and invariable laws which govern matter, supposing it to have a power of motion. Therefore, the history of nature will be the same, upon either supposition. At the same time, as this hypothesis not only contradicts common sense, but, in place of a beautiful chain of causes and effects, discoverable in the operations of matter, presents a deformed and crude scene, which detracts from the wisdom of the deity; I think it of consequence to bestow some thoughts upon it: and, I am hopeful to make it evident, that this hypothesis is no better supported by reason than by experience.

To put an end to a good deal of loose reasoning upon this subject, it will be necessary to ascertain the meaning of the terms *material* and *immaterial*; which, so far as I know, has not been done with sufficient accuracy. All beings and existences must be either material or immaterial; or, in other words, must be matter or not matter: therefore, if we know what is matter, we cannot be at a loss about what is not matter. I take it for granted, that we have no conception of matter but as substance or body, endued with colour, figure, extension, impenetrability, or
other

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other properties of such a nature as to be objects of our external senses. The direct opposite, must of course be an accurate description of an immaterial substance, *viz.* that which has properties of such a nature, as not to lie open to any of our external senses, more than colour to one born without sight. Thus it comes out, that immateriality is merely a negative term, comprehending every thing that is not matter. And it is of consequence to be observed, that the distinction betwixt material and immaterial, not being founded on the nature of the things which are so distinguished, but on the limited nature of our external senses, has not the least tendency to explain the nature or properties of immaterial substances, further than barely, that these properties are of such a kind, as not to be the objects of any external sense.

FROM these premises, the following reasoning will, it is hoped, be found entirely conclusive. Size, figure, motion, weight, &c. are qualities of matter which are perceived by our external senses; but there is none so foolish to maintain, that matter can have no qualities but what are objects of an external sense. It would be the same as to deny the
existence

existence of immaterial substances because these do not exhibit themselves to our senses. Power is a property or quality, of which none of our external senses afford us the perception; and therefore our want of perception of power, does not more conclude a negation of power to matter, than to spirit. In general, we have no means to come at the knowledge of a *cause*, but by the *effect* produced. We cannot, *a priori*, conclude, that animate beings are endued with any sort of powers, more than inanimate. Experience is here our only guide. We find by experience man to be a reasoning being, endued with many powers and faculties: and, by the same experience, we find matter to be endued with certain powers and faculties. Both are discovered by the effects produced; and we have no other means to make a discovery. We see a stone fall without any external impulse. From that effect we have a just foundation to conclude, that the stone has a power of moving itself. And, if we have not a just foundation to make this conclusion, we have not a just foundation to make this other conclusion, that a man has a power of self-motion when we see him walking. And after

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after all, it must appear extremely whimsical to deny to matter a power of motion, merely because matter discovers itself to our external senses in part; when, at the same time, we are so ready to attribute powers of every sort to immaterial substances, which cannot be reached by our external senses, either in whole or in part.

It is suggested above, that to endue matter with a power of acting according to general and invariable laws, exhibits a more beautiful and compleat system, than to leave it absolutely inert, to require a continued interposition of the deity. To illustrate this point, I chuse to borrow the honourable Mr. Boyle's sentiment, whose piety was not inferior to his knowledge. "It seems manifest enough, that whatsoever is done in the world, at least where the rational soul intervenes not, is really effected by corporeal causes and agents, according to the laws settled by the omniscient author of things." And he observes, "That as it more recommends the skill of an engineer to contrive an elaborate engine, so as that there need
nothing

* Inquiry into the vulgar notion of nature. p. 60.

“ nothing to reach his ends in it, but the
 “ contrivance of parts void of understand-
 “ ing ; than if it were necessary that, ever
 “ and anon, a discreet servant should be em-
 “ ployed to concur notably to the operations
 “ of this or that part, or to hinder the en-
 “ gine from being out of order : so, it more
 “ sets off the wisdom of God, in the fabric
 “ of the universe, that he can make so vast a
 “ machine perform all those many things
 “ which he designed it should, by the mere
 “ contrivance of brute-matter, managed by
 “ certain laws of motion, and upheld by his
 “ ordinary and general concurrence ; than if he
 “ employed, from time to time, an intelli-
 “ gent overseer to regulate and controul the
 “ motion of the parts *.” What may be the
 opinion of others, I cannot say ; but, to me,
 this argument is perfectly conclusive. Con-
 sidering this universe as a great machine, the
 workmanship of an intelligent cause ; I can-
 not avoid thinking it is the more compleat,
 the less mending or interposition it requires.
 The perfection of every piece of workman-
 ship, human and divine, consists in its an-
 swering

* Inquiry into the vulgar notion of nature. p. 7.

swering the designed purpose, without bestowing further labour upon it. And therefore, upon the whole, as we have no foundation, either in reason or experience, to deny activity to matter; I conclude, that the doctrine of the absolute inertness of matter, is not only repugnant to truth, but tends, in an indirect manner, to arraign the deity of want of power or of wisdom, or of both.

Of the VIS INSITA or VIS
INERTIÆ

HAVING thus brought our subject within bounds, by evincing that we have no occasion to search for any other immediate cause of natural appearances than the activity of matter itself; I shall proceed to examine some of the powers of matter, such of them as are productive of the most remarkable effects: and shall begin with the *vis insita*, or *vis inertiae*, that signal property of matter, whereby, as philosophers teach, “A body
“always

“ always perseveres in its state of rest, or of
 “ uniform motion, in a right line, ’till, by
 “ some external influence, it be made to
 “ change its state.” This property is handled by writers without any degree of accuracy. The bulk of them resolve it into the inertness and passive nature of matter; and consider the present state of a piece of matter, whether of motion or of rest, to be an effect, which, once produced, must continue to exist as it is, ’till it be changed or destroyed by the operation of a new cause. But, as this conception of the matter is liable to strong objections, which shall by and by be mentioned, the most rational writers admit of a power in matter to preserve itself in its present state; but then, considering the perseverance in rest and in motion, to be effects of the same kind, they, without making any distinction, attribute both to the same power or cause.

I cannot subscribe to either of these notions. It is obvious to me, that the mere negation of a cause, tho’ it may account for the continuance of a body at rest, as it may account for its preserving the same figure or colour; yet can never account for the re-

stance made in the change from rest to motion, or from motion to rest. And it is equally obvious, that a state of motion is very different from a state of rest; and that the preserving a body in motion, must be an operation of a different kind, from that of preserving it at rest.

IN order to handle this subject with some degree of accuracy, I shall endeavour to make out the three following propositions: *first*, That motion is a continued action, which must infer some power continually acting. *Secondly*, That as matter resists a change from rest to motion, as well as from motion to rest, this resistance is not to be accounted for by the mere negation of a cause, but is a positive effect to require a cause as much as motion does. And, *lastly*, after explaining the cause of the continuance of motion, I shall endeavour to show, that it is an effect of a different kind from the resistance which matter exerts against any impulse which puts it in motion.

WITH regard to the first proposition, motion evidently differs from the other properties of matter. It is not like extension, figure, and such like qualities essential to mat-
ter.

ter. Every body must be of a certain size and figure; but it is easy to separate motion from body, by supposing it to be at rest. But what we have principally to consider in the present argument is, that the moulding of a body into a certain figure, or the giving it a certain colour, are not imperfect, but compleated effects; which, once produced, must continue invariably the same, until some power be exerted to make an alteration. This is involved in the very conception of colour and figure, and is so simple and clear a proposition, as to be incapable of any illustration. The bare negation of a new cause, is sufficient to account for the continuance of such effects. Rest is of the same kind. It requires no action or operation to preserve a body at rest, more than to preserve its colour or figure. But motion, being the direct opposite of rest, cannot be one of those compleat effects, which, once produced, must continue invariably the same. Motion, by the very conception of it, is action. While a body is in motion, it is in continual action; and, as action implies power, there must be a power continually exerted to preserve a body in motion. Again, if motion be considered

dered as an effect, it can only be compared to figure or colour, with regard to that part of it which is past : what part remains to be performed, is so far from a compleated effect, that it is not so much as commenced ; and therefore must necessarily be conceived as a different effect, to the production of which there must be a new exertion of power. Motion then is a continued effect, or rather a series of different effects, to require a cause in continued action. Accordingly, when we attend to our own perceptions, we do not inquire for a cause, when a body ceases to move. If the cause of the motion cease, we seek no other reason for the ceasing of the motion. If a thing so evident can admit of illustration, let us figure an animal in motion. It never came into the mind of any person, that a man is passive in the action of walking or running. In this case we reason justly, that walking or running requires a continued exertion of power. It requires no exertion of power to put an end to these motions. The man has but to forbear to exert his power, and rest ensues. In this matter there is no difference betwixt animate and inanimate beings ; not even tho' it should be
supposed

supposed, that matter is altogether passive. For, if matter itself has not a power of motion, the spirit which actuates matter, cannot otherways preserve it in motion, but by a continued exertion of power,

THE second proposition is almost self-evident. To account for continuance at rest, nothing is required except barely the negation of a cause. But matter at rest not only continues in that state, but plainly resists every force which tends to change its state from rest to motion. Matter makes no resistance to any cause which produces an alteration in its figure or colour. Resistance is a positive effect which requires a positive cause. And this property of resistance which all matter is endued with, is a principle directly opposite to inertness or passivity, if I may use that term. Resistance is plainly action, which every man may readily perceive in himself, when an impulse is made upon him: and, were matter absolutely passive and inert, it would make no resistance, but be carried on by every impulse, without diminishing the force of the impelling body.

WITH regard to the third proposition, we cannot be at any loss to assign a cause for the continuance

continuance of that quantity of motion which is once given to a body. If the intervention of spirit be rejected as above, there is but another hypothesis to recur to, which is, That inanimate matter is endued with a power to preserve itself in motion, as well as animated matter is, without any other difference, but that the former having no will or feeling, acts blindly and invariably by an established law. It has no spontaneous motion, because it has no will. Neither has it in every circumstance a power of beginning motion; but, when once set in motion, it has a power given it by the Author of nature to continue itself in motion. And this is a law which regards all matter without exception.

And this power being once established, it remains only to be made out, that it is of a different kind from that which resists a change from rest to motion, and from any degree of motion to a greater. The thing is obvious; for, as causes are best distinguished by their effects, the cause cannot be the same of two effects diametrically opposite, *viz.* preserving a body in motion, and opposing motion. It may serve to clear this matter, if we attend to the distinction betwixt impulse and resistance.

ance. They are simple ideas, and therefore incapable of a definition; but their difference will be obvious, by considering a man in the act of pushing against an obstacle, and barely resisting force applied against him, without pushing again, or counteracting. The man, no doubt, is active in resisting, as well as in pushing; but the actions are very different in their nature, and carefully to be distinguished in accurate reasoning. In a word, the tendency of impulse is, to produce motion; the tendency of resistance is, to prevent it. These powers, therefore, so opposite to each other, shall hereafter be distinguished by different names. The *vis insita* shall be appropriated to that power which is favourable to motion; leaving the other power which resists motion to be denominated by the *vis inertiae*, or rather *vis resistentiæ*.

THO' this *vis inertiae* be a power, as observed, inherent in all matter, there are certain experiments which may appear to clash with it, and which, upon that account, merit attention. If a round ball be laid upon a horizontal table *in vacuo*, in which situation there will be no counteraction of gravity, it is certain, that the smoother the ball and table
are,

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are, the resistance will be the less : and they may be made smoother and smoother, till the resistance be quite insensible. If the resistance do not vanish altogether, it may be owing to our want of art to make any surface absolutely smooth. There is no reason, it may be thought, to ascribe the small remaining resistance to a supposed *vis inertiae*, when it can be accounted for by other causes ; and that it is unphilosophical to suppose the existence of a cause, when we cannot point out a single effect that results from it, and from no other cause.

BUT there is another experiment of the very easiest operation, and which is liable to no ambiguity. Let a body of any determined weight be suspended by a thread or rope fix'd to a hook in the ceiling : the least conceivable force will put this body in motion. If any resistance at all be felt, it ought to be attributed to the density of the air, not to the body. And indeed these experiments infer, no more than what is admitted by every philosopher, that the smallest force is sufficient to move the greatest body. It is acknowledged, that, were the globe of the earth suspended by a chain, there is not a force so small

small which it would not yield to. These, and such like experiments, look as if matter had no attachment to rest, no *vis resistentiae*, but that it readily yielded to motion. That a body is put in motion by the very smallest force, and yet that it resists a change from rest to motion, are propositions which may appear not to be very consistent: yet both are true in fact. And the difficulty will vanish, by attending to the power of the *vis inertiae* or *resistentiae*, that it is not measured merely by the size or bulk of the body to which it belongs, but has a relation to the *momentum* of the impingeing body. The least force will put the greatest body in motion: but from this it does not follow, that matter does not resist a change from rest to motion. It only follows, that this resistance is always less than the *vis motrix*. To calculate the proportion betwixt the *vis motrix* and the resistance of a body at rest, shall be the subject of the next chapter.

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OF

OF THE COMMUNICATION OF MOTION.

THE Communication of motion from one body to another, has been extremely puzzling to metaphysicians; tho' not to practical philosophers, who make no difficulty to suppose, that motion actually passes out of one piece of matter into another. Let us figure one billiard ball at rest, and another directed upon it with a certain velocity. It is understood that part of the motion of the impinging ball passes into that which is at rest, after which it proceeds to move with that degree of velocity which is communicated to it. This account of the matter appears to me altogether absurd. Motion cannot subsist by itself to be transferred from place to place, or from body to body. It is one of the qualities or properties of matter; and it is no less repugnant to the very conception of the thing, that the motion of one
body

body can pass from it to another, than that the figure of one body can pass from it to another. In general, qualities, properties, and affections, are inseparable from the particular bodies to which they belong. They have no separate existence, and therefore cannot be conveyed, even in the imagination, from one body to another. The green colour of this field, cannot be taken from it, and bestowed upon another. All that can be done, is to give the other a similar colour. My ideas or feelings cannot be conveyed from me to any other person, tho' similar feelings or ideas may be produced in that other person. This is not a play about words; it leads to the explanation of a phenomenon which natural philosophers have not been able to explain with any satisfaction. When motion is produced in one body by the impulse of another, there is no necessity to embrace so absurd a doctrine, as that motion passes from the one to the other. The motion produced is easily deducible, from the principles above laid down. The *vis insita* and the *vis resistentie*, are sufficient to account for all the effects that proceed from the collision of hard bodies which have no elasticity. If the resistance

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sistance of a body is always less than the im-
 pressed force, the resisting body, after its re-
 sistance is overcome, must necessarily be car-
 ried along with the body which impinges up-
 on it. And, being once put in motion, its
 own *vis insita* preserves it in the same degree
 of motion. I am here putting the simplest
 case, that of a body in motion, impinging
 upon one at rest; and, for the ease of the i-
 magination, I shall suppose these two bodies
 equal. In this case it is ascertained by expe-
 riments, that the two bodies in contact, go
 on with half the celerity which the impinge-
 ing body had before the collision. For it is
 a general rule in the percussion of hard bo-
 dies, that the *momentum* or force, directed to
 the same point, continues the same after con-
 tact as before. There is no difficulty here:
 for after half of the force of the impinging
 body is destroyed, by the resistance of the
 body at rest; the other half remains, with
 which the impinging body moves, and car-
 ries along with it the other body from rest to
 motion. When the resistance of the body
 at rest is once destroyed, it yields to the im-
 pressed force without any further opposition;

and

and, by its impenetrability, must be carried along with the other.

BUT, in order to form an accurate notion of this operation of percussio, two preliminary points must be settled. The first is, to determine the precise nature of the action exerted by a body at rest when impelled by one in motion. Does the body at rest return stroke for stroke, or impulse for impulse? Does it truly react, or does it barely resist? That it resists only, and does not react, will be evident from a single consideration, which is, that motion is essential to impulse, not at all to resistance. Now, if the body at rest reacts, by returning stroke for stroke; it must rush upon the impinging body, and carry it backwards at least for some small space: which cannot be; because, once admitting a retrograde motion, the *vires insitæ* of the two bodies must carry them on in the same direction, which is contrary to fact. It is obvious, then, that there can be no repercussion or reaction of the body at rest. All the obstruction it gives to the action of the impinging body, must be merely by resistance. The other point to be settled is, whether the operation be instantaneous, or whether it be performed

formed in time? That it is instantaneous, will thus appear. A gradual change of motion supposes, that the foremost body passes through every degree of velocity from rest till it acquires its greatest velocity; and that the motion of the impinging body is gradually retarded in proportion. But this supposition is inconsistent with a known fact, that the bodies continue conjoined after contact; whence it is evident, that both must move with the same velocity from the instant of contact.

THESE preliminary points being settled, the operation may be easily analyzed. Tho' the whole is performed in an instant, it may, however, be distinguished into different steps. The first is, that, by a certain degree of resistance, a proportion of the *momentum* or force of the impinging body is destroyed, and consequently of its velocity. The next step is, that, with the remaining force and velocity, an impulse is made upon the body at rest, which now resists no longer, whereby it begins to move with the remaining velocity of the impinging body. And the last step is, that it is carried on with the same degree of velocity by its own *vis insita*.

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FROM what is said, it will be easy to adjust the proportion betwixt the *vis motrix*, and the resistance of a body at rest. For tho' resistance and *impetus* are so essentially different, as not to admit of a comparison betwixt themselves; yet, the effects produced by them, are capable of a comparison. Now, the force lost by the impinging body is a just measure of the resistance of the body at rest; and if the *momentum*, before and after impulse, continue the same; it must follow, that the *momentum* of the body which is put in motion, is equal to the *momentum* lost by the impinging body. Therefore the *momentum* of the impinging body, is to the resistance of the body at rest, as the quantity of matter of both bodies, is to the quantity of matter in the latter.

THIS power of resistance in matter, which is never exerted but when an impulse is made upon it, is of so singular a nature, as to lead us to suspect some very artful contrivance. It is surprising that writers have given very little attention to the laws of motion, considered in the light of final causes, tho' it is a most beautiful speculation. It would carry me too far from my present purpose, to enter

ter professedly upon this theme. And therefore I shall barely touch upon it, so far as concerns this property of resistance. Gravity, an universal law of matter, serves the noblest purposes. In conjunction with the *vis insita*, it preserves the solar system in order. The *vis insita* is useful in another respect; because, by its means, we can apply force greatly superior to our own. The resistance of bodies again, is of admirable use to keep force within proper bounds. If the smallest body, impingeing upon the greatest, had the effect of communicating its own velocity, which it would do, were there no resistance: the smallest force would be sufficient to give a new violent motion to the earth, and to put the lives of all mankind in the power of every individual.

O F

OF ACTION AND REACTION.

IT is laid down as an universal law of matter, "that reaction is always contrary and equal to action." Or, "that the mutual actions of two bodies upon each other, are always equal, and with opposite directions." And Sir *Isaac Newton*, in illustrating this law, gives particular instances of its taking place in all the different actions of matter; to wit, in impulse, in pressure, and in resistance. For my part, I have always been puzzled about this law, and am uncertain, after all, whether I have a just conception of it. Are we to understand it to be a property of matter in general, that when acted upon, it exerts an equal and contrary action? or only, that in these particular cases where matter reacts, the reaction is always equal and contrary to the action? The former is Sir *Isaac's* sense of this law; which is evident, from his placing it among the laws that govern all matter in general; as well as from

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his various instances, importing that it holds in every case whatever. And now, taking the law in this sense, difficulties croud upon me; of which I shall mention a few, that make the greatest figure. The first is, that, according to this law, there cannot be in matter, such a thing as resistance without reaction; a bold assertion, which contradicts what appears to be the fact in common apprehension; and which therefore ought not to be admitted, without a very compleat induction of particular cases; more compleat, so far as I can learn, than hitherto has been attempted. But, not to rest upon the negative, it appears clear, that, in the collision of hard bodies, there is resistance without reaction: and I flatter myself with having made out above, that, were there reaction in this case, the effects of collision would be very different from what they are in fact. And this leads to another difficulty, that if reaction were, in every case, equal to action, *viz.* in hard bodies as well as in elastic; the effects of collision behoved to be the same in both. Two elastic balls meeting in opposite directions, rebound; because, after their force is spent by collision, the same force is restored
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to each, by their mutual reaction; and the same must happen to hard bodies, if they have the same power of reaction. In a word, I cannot distinguish elasticity from a power of reaction; and a hard body differs from an elastic only in this, that the former has no power of reaction, but only of resistance. A third difficulty is, that, in the bulk of the instances commonly given to illustrate this law, I cannot discover an equality of action and reaction. And lastly, what appears the most unsurmountable difficulty is, that, excepting the case of elasticity now mentioned, and that of fluidity to be mentioned afterwards; I have not found a single instance of an effect produced by the action of one body upon another, but what may be fairly explained by acknowledged principles, laying aside altogether this supposed equality of action and reaction. And if no fact can be condescended on, to prove the point with regard to hard bodies; why should we adapt this as an universal law of motion or of matter? for surely it is irrational to admit of any law or principle without evidence.

THESE difficulties will be best illustrated by going to particulars. And with regard to

Sir

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Sir *Isaac's* first instance, of a man's pressing a stone with his hand; we must distinguish betwixt a moveable and immoveable obstacle. If a man press an immoveable obstacle with his hand, I cannot here discover any thing but mere resistance, which is not to be overcome by the force of the hand. This is an effect which must follow from the impenetrability of matter, without necessity of taking under consideration, any other of its properties. I discover here no reaction. There is no counter-action or impulse in the stone, to correspond to the action or impulse of the hand. There is nothing but resistance, which is indeed an action, but an action which is not exerted upon the hand: the action is exerted within the resisting body itself, to preserve it from motion. And, if this resistance shall be conceived as a counter-action, which it may be in a loose way of thinking; I cannot however conceive any equality betwixt the pressure of my hand and this supposed counter-action of the stone. At this rate, the counter-action of the stone is equal to every degree of impressed force, provided the impressed force be not great enough to move the stone; and so it will be equal at
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the same time to a thousand different degrees of force. To talk of the equality of action and reaction in such a case, means really nothing more than that the hand has not force enough to move the stone.

AGAIN, if the obstacle be moveable, it coincides with another instance given, that of percussio, or the collision of hard bodies. Here indeed it is true, that there is as much force lost by the one body, as is generated in the other. But then this is plainly the effect of the *vis resistentiae* as explained above; and therefore it is unphilosophical to introduce any other principle by which to account for this phenomenon. At the same time, I cannot discover any equality of action and reaction in this case; on the contrary, it is established by infallible experiments, that the impressed force or action is always greater in a determined proportion, than the resistance or reaction. And it is this very proportion which produces the effect of an equality of motion before and after impulse; or, in other words, the acquisition of as much force by the one, as is lost by the other.

ANOTHER instance is of a horse drawing a stone by a rope; and it is said, that the
stone

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stone draws the horse as much as the horse the stone. Were this so, there could be no motion. All I can discover here, is mere resistance in the stone, occasioned partly by its *vis resistentiae*, and partly by friction. And after this resistance is overcome, which spends or exhausts so much of the horse's strength; the stone is drawn along with the horse's remaining strength, without any further resistance.

WITH regard to gravitation, which is given as another instance of the equality of action and reaction; it is very true, that there is an equal tendency in every particle of matter to every particle; but I am at a loss what further can be made of this, than that such is the law of gravitation. Why then should we endeavour to account for this effect by any separate law? I might add, that this at best is a doubtful instance of the mutual actions of bodies upon each other. It will perhaps, upon examination, be found the more safe opinion, that bodies act not upon each other by the power of gravity. But of this afterwards.

KEILL, in explaining this law of nature, seems to confine it to the percussion of
bodies;

bodies ; and rests it wholly upon this fact, that equal changes of motion are produced upon collision; which is, in other words, that, whatever force is lost by the one body, an equal force is generated in the other. And indeed it appears probable, that the law has been invented chiefly to account for this effect: if so, it is extremely rash in philosophers to apply it, as they do, in every case where there is the least appearance of action and reaction, as if it were an universal law of nature that must obtain in every case whatever. It was not discovered, it would seem, that the equality of motion before and after percussion, is the genuine effect of the *vis resistentiae*. Therefore, to account for this phenomenon, a new law behoved to be invented; which, to disguise the matter, must be considered as a general law: for, had it been plainly spoke out, what in effect is said by Dr. Keill, that this law applies only to the percussion of bodies; every person would be sensible, that accounting for the law of percussion in this manner, was doing no more than repeating the fact itself in different words. For to say that the actions of two bodies in percussion are equal and in opposite directions,

ons, is, in an obscure and indistinct manner, really saying no more, than that a force is generated in the one body, equal to that which is lost in the other.

IN accounting for the pressure of fluids *undequaque*, *Muschenbroke* reasons thus: he supposes the water in a vessel to be composed of perpendicular columns of spherical particles, one resting upon another from the surface downwards, and all resting upon the bottom of the vessel. Thus the bottom of the vessel is pressed downwards with the weight of a column: and, because action and reaction must be equal, therefore, says our author, the bottom of the vessel must press upwards with the same force, *i. e.* with a force equal to the weight of the column; whence, says he, the whole pressure upwards, must necessarily be equal to the whole pressure downwards.

So far our author's argument appears conclusive, that, admitting the law of action and reaction, a pressure *undequaque* must be the consequence: and as the law is not confined to fluids, but is supposed to be an universal law of matter; a pressure *undequaque* must not only be the consequence in fluids, but in
solids

solids equally. Here then is a fair *dilemma*. We must either acknowledge a pressure *undequaque* in sand, powder, and indeed in all loose bodies, as well as in fluids; or confine this law of the equality of action and reaction to fluids. Can we remain a moment in doubt betwixt these opposites? We are certain that a pressure *undequaque* is not a property of solids. The equality of action and reaction is but an assertion without evidence. What remains then, but that we adhere to the former, and reject the latter, except as to fluids. And thus our author luckily, tho' without intention, has furnished a very convincing argument against the universality of this supposed law of action and reaction. If an *undequaque* pressure is an effect of this law, it follows clearly, that this law takes not place in solids, at least not universally, as in fluids.

F O F

OF GRAVITY.

GRAVITY is one of the most remarkable properties that belong to matter. By this property, which is universal, every particle of matter tends to every other particle; whereby, if not obstructed, all matter would be gathered into one mass. As every particle of matter has this tendency, the tendencies of any two bodies to a mutual union, must be in proportion to their quantities of matter. And it is observed that the force, which, at different distances, is exerted by two bodies in order to be conjoined, is in the reciprocal proportion of the squares of these distances. It is by this property that the heavenly bodies move round the sun, and are kept from flying off in a tangent: and it is by the same property that bodies left free at any height, fall to the earth with an accelerated motion. When applied to the latter, it is commonly called *gravity*; when to the former, *attraction*. Sir

Isaac

Isaac Newton, to whom the great discovery was reserved, that the descent of heavy bodies, and the curvilinear motion of the planets, are effects of the same cause, chooses to talk of this property of matter with great circumspection and reserve. He pretends only, in his *Principia*, to have ascertained the facts, without venturing to point out the cause. In the general *Scholium*, which concludes that elaborate work, he satisfies himself with having explained "the motion of the
 " celestial bodies, and of the sea, by the force
 " of gravity, without assigning the cause of
 " gravity." He only observes, "That gravi-
 " ty must be the effect of some cause, which
 " penetrates into the very center of the sun
 " and planets, and which acts not in propor-
 " tion to the surfaces, but the solid quantity
 " of matter; its action only decreasing in a
 " duplicate *ratio* of the distances." And adds, "That he has not been able to find
 " out from phenomena, the reason of these
 " properties of gravity, and that he does not
 " choose to deal in hypotheses." It need not be surprising, that this great philosopher should be reserved upon the cause of a theory so extensive and so wonderful, when it
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was his own child. New discoveries are always received with some degree of hesitation; because it is the effect of novelty to produce doubts as well as surprize. But now, that this theory is fully established by habit, as well as by reasoning, and has got a firm hold of the mind; it is not obvious, why later philosophers should affect the same reserve. For my part, I cannot see any difficulty of explaining the cause of attraction or gravity, more than of explaining the cause of a body's continuing in the same degree of motion with which it begins to move. And this I shall now attempt.

It is above established, that the continuation of motion in bodies must be an effect of a power inherent in all matter, of preserving that degree of celerity which is once bestowed upon it. The body, by this power, is directed to move in a streight line. But we perceive nothing in the nature of this power to confine its operation to a streight line, more than a curve. And we may suppose the power still further varied, to make the body, instead of moving in a streight line, or in a curve, to direct its equable motion towards any other given body, within a certain distance.

stance. In general, as we have no means to discover power in any being but by the effects produced; so the nature and operation of the power are only to be discovered by the same means. There is nothing in our conception of any power to confine its operations to one law more than another. And this leads to the cause of gravity or attraction. If we admit of the *vis insita*, the operations of which may be varied in a thousand different ways; it is equally easy to conceive a power in matter by which every particle has a tendency to be united with every other particle. 'Tis but varying the *vis insita* in the two following particulars, to have an exact description of gravity. First, That it have the effect of beginning motion, as well as of continuing it; and, next, That it direct the body to which it belongs, not in a streight line, but towards every body great and small within its sphere of activity.

THE grand difficulty which puzzles foreign philosophers is, to admit of a power in a body to draw other bodies to it; for this is their conception of attraction, suggested by the term itself. They observe, that such a power is inconsistent with a general maxim, that no being can act where it is not; which would
involve

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involve this evident absurdity, to separate the being from its actions. And, from these premises, they justly conclude, that one body cannot act upon another at a distance. It must be confessed, that attraction is an unlucky term, since it has led philosophers into the above mistake; tho' Sir *Isaac Newton* cannot be justly blamed, who made use of a term invented to his hand. By varying the conception of attraction, and by considering it as a power in matter not to draw other bodies to it, but to move itself towards other bodies, the difficulty vanishes.

BUT upon this idea of attraction or gravity, it may be suggested, that there can be no reason, why the power, in exerting itself, should keep pace with the distance of the object towards which its force is directed. The distance of the object, it will be said, can have no effect to diminish the force, when, by the supposition, the action of the one body is not exerted upon the other, but upon itself. This has the appearance of a difficulty, and no more but the appearance. If matter has a power to act in any one case, its actions may be varied by any assignable law. And in particular to imagine a power in a body
impelling

impelling it towards a body at hand, with a greater force than towards one at a distance, is in reality not more difficult, than to imagine it exerting always the same force, without regard to distance.

It is not improbable, that the above mentioned objection, of a body's acting where it is not, has led *Leibnitz* and other foreign philosophers, to adopt the vortices of *Des Cartes*, rather than Sir *Isaac Newton's* theory; liable, according to their notion of it, to an insuperable objection. Yet there cannot be conceived a more whimsical hypothesis, than that of a fluid circulating about the sun, in which the planets are involved and carried along, like a ship in a torrent. The ingenious *Mapertuis*, in his essay upon attraction, moves several objections to it, even as new moulded by later writers. He objects in the first place, that the planets do not move in a circle, but in an ellipse. In the next place, that they do not move with celerities proportionable to their distances from the sun; which they behoved to do, if carried along in a *vortex* moving, like a wheel, equably round the sun. These objections are just; but, in my apprehension, he has overlooked

looked the most solid and weighty objection. Whatever cause can be assigned for the motion of this fluid round the sun, will equally apply to the motion of the planets, without necessity of introducing a new fund of matter, of the existence of which we have no evidence. More particularly, motion is obviously an effect which must have some cause. This cause must either be a continued interposition of a voluntary agent, or a power given to matter to continue itself in motion. A piece of matter cannot move in a straight line, without one or other of these causes; and as little in a circle. Therefore, to make this supposed fluid to circulate about the sun, one or other of these causes must operate; and one or other of these causes is sufficient to account for the motion of the planets, without necessity of inventing a fluid to produce the effect. *Des Cartes* therefore, and his followers, are guilty of the same sort of absurd reasoning, for which we justly laugh at the poor *Indian*, who was forced to invent an overgrown elephant to rest the earth upon, and an overgrown crab to be a footstool to the elephant.

WHETHER

WHETHER the same inconsistency of a body acting where it is not, has moved our *British* philosopher to invent an aetherial *medium* much rarer than air, as the cause of gravitation, I cannot say. In the twenty first query, at the end of his optics, he observes, "that this *medium* is much rarer within the
 " dense bodies of the sun, stars, planets and
 " comets, than in the empty celestial spaces
 " between them. And, in passing from them
 " to great distances, that it grows denser and
 " denser perpetually; and thereby causes the
 " gravity of those great bodies towards one
 " another, and of their parts towards the
 " bodies; every body endeavouring to go
 " from the denser part of the *medium* towards
 " the rarer." I am not better satisfied with this hypothesis than that of *Des Cartes*. For, without losing time upon a minute enquiry into the very peculiar properties with which this supposed aether must be endued, to produce the effects assigned it; the same objection lies against it, that is above urged against the vortices of *Des Cartes*, that it is a new species of matter invented without evidence, and indeed without necessity: for it is as easy to endue the planets with a power

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which impells them towards the sun, as to endue this supposed aether with a power which repells it from the sun. Therefore the argument which is justly urged by this author himself against a *plenum* in the 28th query, at the end of his optics, may be urged with equal success against this supposed aether, "It is of no use; and, as there is no evidence for its existence, it ought to be rejected."

I Must own, at the same time, great reluctance, to embrace a doctrine which overturns, or seems to overturn, the most beautiful part of Sir *Isaac*'s own theory, and that which affords the strongest conviction of its truth, viz. the connexion, by a common cause, betwixt the curvilinear motion of the planets, and the descent of bodies towards the center of this earth. Supposing an aetherial *medium* to be the cause of the former, it cannot well also be the cause of the latter. Among other reasons, this occurs, that the different densities of the supposed aether, on the opposite sides of a bit of matter left free in the air, must be, *quam proxime*, nothing. This must be yielded; and the consequence is, that the bit of matter involved in

a *medium* which presses equally *undequaque* or *quam proxime*, will either remain at rest, or move with a very slow pace. But this is contrary to fact; for the acceleration of falling bodies, within the least perceivable distance, is perceivable.

THE power of gravity has a singular property not commonly attended to, that its force is not spent by being exerted upon any single body. Its whole force is exerted upon a multitude of bodies, all at the same time. Thus the earth tends at once both to the sun and moon; and the whole force of its tendency is directed to each, just as much as if the other were annihilated. Thus the sun is impelled towards the whole system of planets and comets; and its total impulse is directed upon every one of them at the same instant. It cannot fail to appear singular and surprising, to find a force exerted in so many different and even opposite directions at one time, without being diminished by division. Yet all this proceeds from one single property, that every particle of matter tends to every other particle; and consequently, by encreasing the quantity of matter indefinitely, the sum of the tendencies of any one particle

particle of matter, may be greater than any assignable quantity. But nature is wonderful, as well as various, in her operations; and we ought to subdue our incredulity, when, upon searching into her works, we find so many appearances different from what we are accustomed to in common life.

HAVING thus established, that there is a power inherent in matter, by which every particle tends to be united to every other particle, and which power is in continual action without ever being suspended but for a moment; the curvilinear motion of the heavenly bodies, as well as the descent of bodies towards the center of the earth, are obviously the effects of this power. With regard to any particular planet, the earth for example, we have only to suppose, that it is once set in motion by the hand of the Almighty; it will endeavour to preserve itself in the same degree of motion by the *vis insita*. Having likewise a tendency to the sun, these two powers constantly acting in different directions, carry it round the sun. At the same time, the proportion of these two forces is so adjusted, that the earth, as well as all the other

other planets, describe an elipsis, in one of the *foci* of which the sun is placed.

IN the descent of heavy bodies towards the center of the earth, the force of gravity is supposed to be invariable. For, tho' this force lessens by distance, yet the distance of any two points, from which we have access to drop a body, is so inconsiderable in respect of the distance of either from the center, that it is left out in all calculations as imperceptible. Hence the following question is readily suggested, How comes it, that the power of gravity which in this case is supposed invariable, has the effect of accelerating the motion of a falling body? It should be thought, that the power of gravity, supposing it invariable, must produce an equable motion downwards, without acceleration; just as the *vis insita* does, laying aside gravity, in whatever direction the body is thrown. For in general, as effects must always correspond with their causes, every force which is uniformly exerted without diminution or augmentation, must produce an equable motion, without acceleration or retardation: and, on the other side, every varied effect which

which is gradually diminished or augmented, must proceed from a varied cause.

THIS difficulty is endeavoured to be explained in the following manner. Gravity, it is said, has a remarkable property, "That it acts with the same force upon a body that is already in motion, as upon a body that is at rest, so as to produce equal accelerations in falling bodies in equal times *." But this manner of conceiving the effect of gravity, whatever obscurity it may occasion, tends not to remove the difficulty. One thing is extremely clear, that we have no means afforded us, to estimate the force of any power, other than the effects produced by it; which holds in general, whether the power be internal or external. From the uniformity of the effects, we conclude the *vis insita* to be a uniform power; and we have the same foundation, from its varying effects, to conclude gravity to be a varying power. In short, when acceleration is produced by an external cause, it will be evident to every one, that the force of the impulsive cause, must be continually increasing, to produce such

* M'Laurin's account of Newton's philosophy, p. 248.

such an effect; and it will be a hard task to assign any just foundation for a difference in this particular, betwixt external and internal force.

WE must therefore yield to the difficulty, because it is unsurmountable. It is very true, that a body has, *quam proxime*, the same weight at the distance of ten foot from the surface of the earth, as at the distance of a hundred: and the force of gravity is, *quam proxime*, the same when a body is dropt from the hand at these different distances. But then, tho' gravity is supposed invariable, where the difference of the distances is so small; yet this only holds where other circumstances are the same: that is, where the body is either at rest, or moves with the same celerity. For it is extremely clear, that, in the perpendicular descent of a body, the force of gravity varies every instant, and turns greater in its progress downwards. And indeed gravity cannot otherways produce acceleration, more than the *vis insita* does. Nor is there any thing singular in this property of gravity. The *vis insita* has a property some what similar: for, tho' it continues invariable after the body is set in motion,

tion, yet it is not always the same. It keeps pace with the impressed force, and is very different, according to the different forces with which bodies are put in motion. In a word, since effects must always correspond with their causes, the gradual increase of the force of a falling body, must infer the gradual increase of the power of gravity, which is the cause of the motion. Or rather, considering the matter in a different light, perhaps more accurately, the tendency or *nifus* of a body towards the center of the earth, which is greater in motion than at rest, and which gradually increases with the velocity of the motion, being nothing else but the exertion of its power of gravity, makes it evident that the power of gravity is continually increasing from the beginning, to the end of the motion.

IN accounting for this phenomenon, I have neglected the *vis insita*, tho' it may probably act in conjunction with gravity in the descent of bodies towards the center of the earth, as well as in the curvilinear motion of the planets. For, if the force of gravity be supposed invariable, the addition of the *vis insita*, which is also an invariable force, will only

only bring out a greater invariable force, which can never produce an acceleration of motion. The acceleration therefore must be attributed to gravity alone, the force of which, in the beginning of the motion, is supposed to be less than any assignable quantity, whereby a body, set in motion by the force of gravity, passes through every degree of velocity from rest, till it acquire that velocity which it has when it touches the ground.

I have dwelt the longer upon this property of gravity, because there is connected with it another property, which is, that in motion, the action of gravity is not to be considered as one action exerted through a length of time, but as a number of different actions exerted incessantly. For, if the gravity of a body in motion continues not the same any two successive moments, but is continually varying, the action must vary with the power; and consequently is not one, but a number of different actions. Gravity in a body at rest, is, like the *vis insita*, one invariable power, which produces one invariable action exerted through a length of time. But, when once the body is put in

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motion, we must necessarily admit, first, that gravity is exerted by repeated impulses; and next, that these impulses, continually turning stronger, form an increasing series, holding an exact proportion with the growing velocity, of which these impulses are the cause.

AND this leads us readily to conceive the operation of gravity in the ascent and descent of bodies. A stone, in its perpendicular motion, is carried upwards by its *vis insita* counter-acted by gravity: in its return, it is carried down by gravity without any counter-action. At first view, the ascent will be considered as the operation of two powers acting in opposite directions; the effect of which commonly is, to produce an equable motion with the difference of the forces. And, were the action of gravity one uniform action, like that of the *vis insita*; such would truly be the effect: but, as gravity is exerted by repeated impulses, a different effect must follow. The very first impulse of gravity takes off from the force of the *vis insita*; what remains is lessened by the second impulse; and so on, till the *vis insita*, gradually decreasing by the repeated counter-actions of gravity, be annihilated altogether. In the
return

return downwards, gravity being exerted without an antagonist, every new impulse produces an acceleration of motion ; and, these impulses being repeated incessantly, the acceleration goes on, till the stone touch the ground.

It remains only to be observed, that, as the force of gravity is accurately proportioned to the velocity, gravity must produce the same effect, whether its force be exerted in the direction of the motion, or contrary to it ; and that the acceleration in the former case must be equal to the retardation in the latter, in any two points where the velocities are equal. Thus, upon the whole, the gradual retardation of upward motion, is equal to the gradual acceleration of downward motion. The same series of numbers represents both ; a progress from the smallest to the greatest number being similar to acceleration ; the contrary progress, to retardation. And hence it is that these two contrary motions are performed precisely in the same time.

It is now easy to account for the different velocities of a planet in the different points of its orbit. The motion of a planet may
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be justly represented by perpendicular ascent and descent. A planet moving from the sun, resembles a stone moving from the earth upwards; and, in its return towards the sun, resembles the same stone returning to the earth, after its upward motion is spent. And, indeed, by prolonging the greater axis indefinitely, an ellypse coincides at last with a streight line; and the motion in the former coincides with the motion in the latter. Considering then that gravity and the *vis insita* are the two powers which govern motion in the curve as well as in the streight line; it must follow, that the law of motion is the same in both.

OF

OF THE FORCE OF BODIES
IN MOTION.

IN explaining the first law of motion, that every body perseveres, as much as in it lies, in its present state of rest or motion; I have touched at that remarkable power which all matter is endued with, of moving on with its original velocity; and to which power I have chosen to confine the term *vis insita*. I now proceed to unfold some further properties of this power. It appears clear, that the *vis insita* might have been so ordered, as to produce an accelerated or retarded, instead of an uniform motion. And it might have been so ordered as to yield to the smallest opposing force, which is the case of the *vis resistentiae* when the body is at rest. There is no difficulty to conceive a body endued with such a property, as to preserve itself in its original degree of motion, where there is no obstruction, and yet to yield to the

the smallest resistance or opposing force. But the *vis insita* is plainly not of this nature. The body not only preserves itself in motion where there is no resistance, but exerts a certain force against every thing that resists its progress. And it is with a view to this force exerted in motion, as well as with a view to the resistance exerted at rest, that Sir *Isaac Newton*, in his third definition, describes the *vis insita* to be “that power in matter, by which every body, as much as in it lies, perseveres in its present state of rest or motion.” The force, however exerted in motion, is essentially different from that exerted at rest. The latter is mere resistance: the former is an impulsive force, by which the body endeavours, *quantum in se est*, to overcome every obstruction to its motion. The force of resistance is above ascertained, and a rule laid down for estimating it, about which all philosophers are agreed. They differ widely about the impulsive force of the *vis insita*: the *Germans* holding it to be as the squares of the velocities; the *English* and *French* holding it to be simply as the velocities. The dispute is spun out to a great length: the parties, as is usual, being more solici-

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tous to enforce their own arguments, than to give fair play to those advanced by their antagonists. And, by profusion of writing, the point is rendered so perplexed and intricate, that there appears not a better way to come at the truth, than neglecting the arguments on both sides, to apply directly to facts for a solution, as one would do upon a point newly started.

CONSIDERING the matter abstractly, I cannot find that there is any inconsistency in the *German* hypothesis. As we may conceive the *vis insita* to be exerted according to any assignable law, so we may conceive it to be exerted in any assignable degree. When we consider the *vis insita* as employed in preserving the same degree of velocity, it follows necessarily, that its force, which is the cause, must be measured by the velocity, which is the effect. But when we consider another branch of its operations, *viz.* its *nifus* to overcome every obstruction to the equable motion, we have no other means to estimate this *nifus*, but its effects; for, however probable it may be, it does not necessarily follow, that this *nifus* must also be measured by the velocity. Facts and experiments, then, are the only means

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means by which we can determine this controversy ; and, when we look about for facts and experiments, there can be none less liable to exception than those, where the *vis insita* of one body is opposed to the *vis insita* of another, by collision and percussio. If a pendulum of one pound weight be let down from such a height, as to acquire at the lowest point the velocity of 10, it will exactly balance a pendulum of two pounds weight, let down from such a height as only to acquire the velocity of 5, and the two bodies will be at rest upon their collision. The remaining at rest upon contact, is compleat evidence, that the forces of the two bodies are equal ; consequently that their forces are as their quantities of matter multiplied upon their velocities ; and therefore that the force of the same body is always as its velocity. A thousand different experiments of the same kind, make it evident, that the force of every moving body is in the precise ratio of its velocity. And those experiments must be absolutely unexceptionable, in which we compare the force of one body, with the force of another equal body moving with a different velocity ; for this is in effect comparing the different

ferent forces of the same body moving with different velocities.

IT being thus ascertained, that the force of a body moving by the *vis insita* is always in proportion to its velocity, we have reason to conclude, from analogy, that the same rule holds where bodies move by the force of gravity. And accordingly this is also made evident from a course of unexceptionable experiments.

THE *German* philosophers, in their experiments, have been misled, by ascribing to the force of the moving body, effects which obviously result from other causes. Not to lengthen out this paper too much, I shall satisfy myself with giving an instance or two. It is a fact agreed upon, that a pendulum swung upwards with a double velocity, will reach a quadruple height. Hence it is inferred by *Leibnitz*, and his followers, that the forces must be as the squares of the velocities; seeing the effects produced by the different forces, *viz.* the spaces gone through, are as the squares of the velocities. And could it be justly maintained, that the space run through is purely the effect of the momentum or force, with which the body is thrown upwards, the argument would be

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conclusive. But it is not so. Laying aside gravity, and the resistance of the air, a body thrown upwards with the smallest force, will move on *in infinitum*. It is by the operation of gravity, and the resistance of the air, that motion ceases, when the body arrives to a certain height. The retardation, therefore, of motion, in bodies thrown up with different velocities, laying aside the resistance of the air, may be a measure of the force of gravity, of which it is the effect; but can never be a measure of the force with which the body is thrown up, of which it is not the effect. And, from the fact of a body's arriving at four times the height with double the velocity, to infer, that the momentum, at its out-setting, must be as the square of the velocity, is really not more just, than to infer, when one body is let drop from four times the height of another body, that it must acquire four times the force of the other body, tho' it acquire but double its velocity; which does not afford the shadow of an argument. When a body is thrown up with a double velocity, and consequently with a double force, the reason why it ascends four times its former height, is plainly this, that the counteraction of gravity, while it has a double force to struggle

struggle with, has but half the time in any given space to produce its effect ; and therefore this body, before its motion be totally subdued, must ascend four times the height that it ascends when thrown upwards with the single velocity and single force.

BUT the argument which the *Leibnitians* trust most to, is founded upon experiments of the falling of balls upon clay, or other soft body ; where it is established, that the impressions made, are in proportion to the heights from whence the balls are let fall, and consequently to the squares of the velocities. From these experiments it is inferred, that the forces must also be as the squares of the velocities ; it being taken for granted, that the impressions made upon the clay must be the measure of the forces or momenta, of which they are said to be the immediate and direct effects. The error of this reasoning is of the same kind with the former. The retardation of the motion of a body falling through a resisting medium, is not the effect of gravity, and therefore cannot be the measure of its force. It is the measure of the resistance of the medium, because it is the effect of that resistance. All the world knows, that when bodies move through a fluid, or any
soft

soft matter, a double force produces a quadruple effect; which may be accounted for in the same manner with the retardation which happens in the ascent of heavy bodies. A double force makes the body ascend four times the height before it be stopt by the counteraction of gravity. And it is equally reasonable to suppose, that when bodies fall into resisting mediums, a double force will carry the body a quadruple space downwards before the motion be quite stopt by the resistance of the medium.

I shall conclude this essay with the following observation, that the several powers I have ascribed to matter, are in nothing similar to occult qualities. The error of those who dealt in the doctrine of occult qualities was, in attributing every different effect to some quality or cause confined to that single effect; which was in reality saying no more, than, what all the world knows, that every effect must have a cause. This was not advancing a single step in knowledge, but amusing one's self with words in place of things. The powers I have attributed to matter, resolve into general laws, each of them productive of a thousand different effects. And we have

no reason to doubt that mere matter may be endued with various powers, as well as animals are. When we say, that seeing, hearing, tasting, touching, smelling, proceed from so many different powers or senses, no person considers this as ascribing effects to occult causes. Have we not the same reason to conclude, that there is a power in matter to continue itself in motion, another power to resist a change from rest to motion, and a third power to unite itself with every other piece of matter; when we perceive effects which as directly result from these powers, as seeing does from the sense of seeing. It may be true, for ought we know, that there is some more general principle in matter, which is the foundation of all these powers. But still it is gaining ground, and knowledge, to trace effects to their causes, and to discover that many different effects proceed from the same cause. To be dissatisfied with such discoveries, merely because of the possibility of other causes still more general, which lie hid from us, is in effect to be dissatisfied with all knowledge whatever; because, however far we penetrate, we never can be certain, that we are arrived at our journey's end.

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ARTICLE II.

Some Remarks on the Laws of Motion, and the Inertia of Matter ; by JOHN STEWART, M. D. Fellow of the Royal College of Physicians, and Professor of Natural Philosophy in the University of Edinburgh.

THE Laws of motion, as delivered by Sir *Isaac Newton*, are all founded on the supposition, that body of itself is absolutely inactive. And inactivity is now commonly ascribed to matter as one of its general properties; body being defined to be whatever is extended, impenetrable, divisible, moveable, and inactive. At the same time, every one knows, that active powers are continually employed through all the parts of nature. The life and motion of animals, the production and growth of vegetables, the attractions of gravitation and cohesion, with other instances of the same kind, are always present to our view.

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PHILOSOPHERS who assert the inactivity of matter, ought therefore to be able to give some good reason, why they refuse to allow it the free possession of such powers in its own right; and why it is only to be regarded as a passive instrument, under the direction, and subject to the dominion, of some superior being. But, in order to ascertain the true notion of the *inertia* of body, the proper method is to begin with the simplest case; and to consider body as a lifeless inanimated mass, without weight, attraction, repulsion, or any tendency to begin motion, till acted upon by some foreign external cause. And surely it would be somewhat surprising, if body, even in these circumstances, should be found to discover any activity.

It hath been imagined however, by some people, "That, if body were utterly unactive, "the smallest force would be sufficient to "move a great body and a little body, with "equal velocity: and that the same power "might communicate a great velocity or a "small velocity to any body.*" Mr. *De Mailran* in *Mem. de l'Acad.* 1728, seems to enter-

* Essay I. p. 9. 21. 24.

tain the same opinion, when he asks, if the *inertia* of matter (which he explains to be that resistance which body makes to its being drawn out of rest, and receiving a determined motion, and which is more or less in proportion to its mass) may not be the effect of some motion; and if it ought not to be conceived as an actual force, acting by some secret mechanism. And the author of an *Essay on Spirit* lately published, maintains in like manner, that there is an active resistance to the beginning of motion in every body; tho' indeed he attributes that activity to an immaterial cause. And Mess. *Buffon* and *Needham* too, who have carried the activity of matter to the highest pitch, have drawn one argument in support of their system, from the resistance of body to motion. *Needham's Observations*. p. 435.

To give the question a fair examination, we shall, instead of speaking about matter or body, suppose for once, a substance before us quite inactive of itself, which is extended, impenetrable, finite, and consequently moveable. Let us try if any less resistance to motion can be expected here, than is actually to be met with from common matter.

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Place any mass of such a substance at rest. It cannot begin motion of itself by the supposition. But an active animated being, as for example, a man, can move it. Some effort must certainly be made, some power exerted, to produce this effect. It will never be pretended, that the same effort can move the mass either with a great velocity or a small velocity; that being as absurd, as to say that a great velocity and a small velocity are one and the same thing. In like manner, it must require one effort to move a small quantity of this substance with a certain velocity, and a different effort to move a great quantity with the same velocity. The same energy of the agent, will never serve to move a given quantity of this substance, or double the quantity of this substance, with the same velocity; no more than it will move the same substance with different velocities. By this exertion of our own activity, we acquire the ideas of forces. The animated being (or the mind) is differently affected by different objects, whether of the senses or understanding. And why should it be affected in the same way, when a great sub-

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stance and a little substance are moved by it, or when a great velocity and a little velocity are imparted to the same substance? When we endeavour to communicate motion to such a substance, we must be conscious of some kind of *feeling*; and these *feelings* must be different in different cases. Thus the idea of *resistance*, as it is called, to motion, in the most inactive substance we can imagine, would be suggested to us from these *perceptions*; and is precisely the same with what we experience daily in handling of matter. Nor does it seem possible to conceive an extended, impenetrable substance, divested of this kind of resistance from *inertia*. The larger the substance is which we intend to move with a given velocity, the greater force must be applied: and, could we suppose it actually infinite, no finite force could move it at all.

THERE is a very extraordinary passage in Mr. *Mcclaurin's Account of Sir Isaac Newton's Philosophy*. p. 100. which, in respect of the high merit of the author, deserves our attention. It is there said, "That, for ought we know, matter may be of kinds so different from each other, that the solid elementary

“mentary particles of the one, may have a “greater *inertia* than equal solid elementary “particles of the other kind.” This conjecture, if true, would effectually overturn what hath been now advanced. But it appears to be equally inconsistent with his own account of *inertia*, and occurs only in a posthumous work. The *inertia* of body is a negative quality, or a negation of all positive power, and therefore can admit of no degrees of greater or less in a given quantity of matter. Two bodies, or two elementary particles of equal quantities of matter, if they are inert at all, must have equal *inertia*, or require equal forces to move them with equal velocities. And, in every case, the *inertia* is proportional to the quantity of matter.

If a body left at rest does not begin motion of itself, it is determined to remain in that state, not from any real repugnance to motion, which is as conformable to its nature as a state of rest: but because nothing is done without a cause. And, when acted upon by any external influence, it obeys without reluctance; the motion produced being in exact proportion to the moving cause.

cause. It has a constant susceptibility of motion, and a perfect facility in receiving it. But we may as well ask, why an inactive substance does not begin some degree of motion of itself? as, why different powers are requisite to produce different motions? When people talk of the *resistance* of matter at rest, as of an *active power*, struggling against any agent, and actively opposing it, they surely frame to themselves some notion of force antecedent to all experience; and, they would do well to inform the world, in what manner this idea was suggested to them.

THE *passive nature* of body is abundantly manifest, from its yielding to the least conceivable action. The leg of a fly moves the whole globe of the earth. A man indeed cannot roll a tun so fast as he can a tennis-ball: and we may find a horse able to draw a loaded cart two miles in the hour, who cannot be prevailed upon to draw it four miles in the same time. But are not such common *phaenomena* as these more naturally accounted for, from the sluggishness or inactivity of matter, than from its supposed activity? A great body set in motion is one effect; a little body moved with the same
velocity

velocity is another. A given body moved with a great velocity, is one effect; and, when moved with a less velocity, it is a different effect. The old principle seems to apply well enough in this case, that effects are proportional to their causes.

It must be confessed, that authors in treating of the *inertia* of matter, and of its resistance to motion, are very apt to express themselves in terms that import a real activity; and which, if strictly understood, are inconsistent with *inertia*. Thus, when a person in a boat pulls a rope that is fastened to the shore, it is commonly said, that the man acts upon the shore in one direction, and that the shore, by its reaction in the opposite direction, pulls the man and boat towards it. Yet, notwithstanding such expressions, will any one seriously maintain, that the ground has an active power to produce any such effect? It is evidently the force of the man extending himself, that draws the shore with his hands one way, and at the same time pushes the vessel with equal force with his feet the opposite way. In like manner, if a person standing in a boat, pushes against the ground with a pole, it is not really the reaction

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ction of the ground that makes the boat recede; it is the force of the man that is the only moving power, acting equally upon both the boat and the ground. When a boat is rowed with oars, the oars are said to act upon the water in one direction, and the water to react upon the oars in the opposite direction, and to produce the motion of the boat. Yet it is plain, that the motion is intirely produced by the rower, who, in so far as he contributes to the motion of the boat, employs the oar as a *lever* of the second kind, where the body to be moved, or the boat, is placed betwixt the moving power and the *fulcrum*.

IN the common account of the progression of animals, whether it be of walking, flying, or swimming, *Borelli* and all other authors content themselves with telling us, That the animal acts upon the ground, air, or water, in one direction, and that the reaction of the medium carries the animal forwards in an opposite direction. This is at best but too concise, not to call it a false theory: seeing it must be manifest that the medium, by any reaction which takes place here, can only consume as much force as is impressed

impressed upon it, and is utterly incapable of producing any kind of motion. The only immediate cause of the motion, being the active force of the animal which presses the medium one way, and its own body the other way, as might be particularly shown in every kind of progression, if it were thought needful.

Thus, upon the most attentive examination into the state and condition of body at rest, we have found it to be perfectly *inert* or inactive. Body in motion next demands our consideration. Supposing a body once put in motion by some agent or other, it behooves us to follow it out, and enquire what happens to it on this change of its state. An extended, impenetrable, inactive substance has received a motion: Will it instantly stop when the immediate influence of the active power ceases? or will it persevere in its new state?

FROM what commonly happens in the motions of bodies, their gradual loss of force and returning to a state of rest; people contract an early prepossession, and are ready to imagine, that rest is the proper state of body. But a diligent review of all the circumstances, soon discovers, that body is equally indifferent

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different to either state, of rest or motion; Body at rest does not change its state of itself; no more does body in motion. A body at rest requires no cause of its continuance; as little does a body in motion. This property of body, by which it perseveres in its state of rest, or of uniform motion, in a streight line, unless when forced to change it by some external influence, is called the *inertia* of matter.

WE know not how motion is originally communicated at all; we know nothing of the nature of motion or force, but by experience. We can define neither; the ideas being quite simple. Natural philosophy however takes it for granted, that such a thing there is. Let a motion or force begin any way you please, we never see it cease till it be destroyed. Why then should we imagine a body ought to stop of itself; and that, to preserve it in motion, a constant exertion is necessary, like that which produced it at first? What argument can lead us to ascribe such an activity to body? What should determine the body to stop, if there be nothing to oppose its motion? Is there any experiment pointing that way? yes, "we
" may

" may be desired to reflect on what a person
 " feels within himself in walking, during
 " which a repeated activity is exerted to con-
 " tinue the motion *." But this, surely, can
 only be intended as an illustration of what is
 meant by the supposed activity of matter in
 motion, and not as a proof of its reality. For,
 every day's experience must teach us on the
 contrary, that it requires a great activity,
 sometimes more than we are masters of, to
 stop a begun motion in our bodies. How
 doth it appear, that the same effort is neces-
 sary to be continually exerted, which was
 employed at the beginning of the motion?
 We find a certain effort necessary to begin a
 motion in our own bodies; but, we should
 find no occasion for repeating it, were it not
 consumed or wasted upon other bodies. When
 we give ourselves one push forwards upon a
 smooth surface, such as ice for example,
 there is no need for a second immediately;
 and, were there no attrition nor resistance
 from the air, the motion would continue for
 ever. If a body set in motion, were to stop,
 retard, or any way change, its motion of it-
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* Essay I. p. 20.

self; that would betray an inclination or tendency to one state preferably to another; it would no longer appear equally indifferent to either, in which alone passivity consists.

THE most general law of matter we discover by experience is, that every effect continues till destroyed by something. Why then may not motion continue till it be destroyed, as well as the magnitude, figure, colour, or any other property of body? or even as well as the very existence of matter? "Motion
" is indeed a mode of existence different
" from all others, nor can we compare it to
" any thing that is not motion *." But that is no reason why it should change of itself, any more than these other modes. If the continuation of motion bore a nearer resemblance to the continuation of any other effect, would that render it any plainer? Motion is an effect *sui generis*; but we have an infinite variety of examples of its continuance. An active being presses forwards a certain quantity of an impenetrable inactive substance: is there not some effect produced here? something then communicated to it? The body is put
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* Essay I. p. 18. 19.

in motion : why ought that motion to cease without a cause ? Action is necessary to the production of motion, and no wonder if nothing can destroy the effect of one action but another action. The trite maxim, *sublata causa, tollitur effectus*, is not to be so literally interpreted, as that an effect may not continue, after its cause ceases to act.

As we know so little of the nature of that influence by which one being puts another in motion, or by which it excites motion in itself ; with what reason can we positively conclude its effects to be only momentary, when they every where appear to be so permanent ? When motion is begun in any body, we attribute it to a certain action which we cannot possibly explain ; when a body continues in the motion which it has once received, it appears to be a necessary consequence of that action. We commonly indeed say, that the body then moves on of itself ; but the only question in debate is, Whether there be occasion for a continual recruit of force, new impressions, new sollicitations to motion, like to what were at first exerted by the moving power.

UNLESS

UNLESS this account of the continuation of motion can be shewn to involve some absurdity, why should we seek for any other cause of it, than the force impressed by the external agent? We may be told perhaps, "That motion is a continued action:" and therefore body continuing to move, is active. But is not this either a begging of the question, or taking hold of the ambiguity of language to support it; instead of explaining, why a motion once excited should cease of itself? No doubt, we commonly say in the mechanical philosophy, that one body acts upon another by impulse, and the other reacts upon it. Nay, Sir *Isaac Newton* himself speaks of matter acting by *inertia*, or *vis inertiae*, which, translated literally, would seem to import an impotent power, or active inactivity. Such terms cannot easily be avoided, without introducing endless circumlocutions. If more accurate expressions can conveniently be substituted in their place, it would be no disservice done to philosophy. But we are not to dispute about words, when the meaning is clear; or, to confound things entirely distinct, because they happen frequently to be called by the same name. The

true

true distinction between active force, properly so called, and the *vis inertiae*, seems to consist in this, that some beings can begin motion where there was none before, either in themselves, or in the body to be moved: in other beings, the motion, when begun from some external cause, is continued for want of a power to stop it. The first of these may be properly denominated *active beings*; the other, even when in motion, have no proper activity of their own.

THERE can indeed be no action upon matter without a production of motion, or at least a tendency to it. But it doth not therefore follow, that motion and action are synonymous terms. Why may not an active being communicate motion to matter, without moving itself? Is not that every whit as easy as self-motion? But, whether the agent moves itself or not, it must have a power of acting, previous to all motion; otherways it would be impossible for matter or any other being to begin motion of themselves. A due attention to this remark, will afford a direct answer to what hath been so much insisted on, "That motion is action." Motion is not action, but the effect of an action.

Activity

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Activity may be variously applied. An action may be with-held by an opposite and equal action, as in the case of two contrary pressures; or, it may be employed in destroying the effect of some former action, as when it stops or retards a moving body. But the genuine characteristic of an active being, is a power of beginning motion either in itself or another, without the means of preceeding motion *. Thus a man from a state of rest can begin a motion which shall move another body: or he can begin a motion which shall stop or retard another motion. And it will readily be admitted, that whatever active being can thus begin motion in another, without the means of previous motion, will of course be able to consume motion in another, without receiving any itself.

WHEN one body strikes upon another and moves it, we commonly indeed call this an action: yet there is no resemblance between this mechanical communication of motion, and

* *All mere mechanical communications of motion, are not properly action, but mere passiveness, both in the bodies that impell, and that are impelled. Action is the beginning of a motion where there was none before, from a principle of life or activity. Clarks Lett. to Leibnitz. p. 327.*

and the activity before described. This is conceived to be an action in a secondary sense. It is only a consequence of the first action. A motion once produced continues, till it be destroyed by an equal and opposite motion, or an action capable of having produced it.

THE effect produced by an agent is a certain quantity of motion or force, which, like other effects, continues without any tendency to perish of itself. This quantity of motion, is always proportional to the real space described by the whole quantity of matter; and that space having length, breadth, and thickness, is measured by multiplying the quantity of matter, by the length of space described by every particle, that is, by the velocity. If a body in motion strikes directly upon another at rest, the two move on as one body after the stroke. They cannot move together with the same velocity that the single body had before the stroke, for then there would be an increase of quantity of motion. And whence should that proceed? such an appearance as that, would discover an activity in bodies indeed. Upon the supposition of perfect inactivity, the quantity of motion must

must remain unchanged: and we here abstract entirely from elasticity, whose effects proceed from a real activity, and have no relation to what is called the *vis inertiae*. To find the length of the space described by the impinging body, we divide the space by the quantity of matter, which is as it were the base. And to have the length of the second space described by the two bodies conjointly, we must divide the same space by the mass of matter in both. As the divisor increases, the quotient must diminish in the same proportion.

WERE we indeed to examine very minutely the gradual communication of motion from one body to another, we shall find a real activity concerned in the operation; but then it is not of that sort against which we are now contending. It is that active force known by the name of the *attraction of cohesion*, which there takes place. When any part of a body is pulled or struck upon by any moving power, it would necessarily be separated from the other parts, were there no cohesion. But, where there is a cohesion, that attractive force must oppose the moving power. And if either the cohesion be strong,
or

or the velocity of the moving power but small, this opposition must continue, till such time as all the parts of the body have acquired one common velocity. That part of the body which is immediately acted upon by the moving power, is first put into motion; and, drawing the other parts after it, gradually communicates motion to them, without ever getting beyond the reach of their attraction, or being broke asunder. Yet, it is still to the moving power that the motion of the whole body must be ascribed; because the attraction among the small particles being mutual, will oppose the motion one way, as much as it promotes it the other; and so can neither forward nor obstruct the motion upon the whole. The attraction serves only to connect and link the several parts together. The force lost by one body, is precisely equal to what is gained by the other; the whole effect or change, with regard to motion being the same, as if it had been instantaneously produced. And there is no argument to be drawn from hence, to prove the active resistance of one body at rest, to another in motion, or the tendency of any body in motion, to return to a state of rest.

M

FROM

FROM the experiment of Mr. *Poleni* we learn, That spheres of equal diameters falling upon soft clay, make equal impressions, when the products of the quantities of matter into the heights from which they fall, are equal; that is, when the quantities of matter multiplied by the squares of the velocities are equal. As it is much easier to determine when two impressions are equal, than when they differ in any proportion of magnitude; the velocities of the different bodies in this experiment, are so adjusted, in respect of the quantities of matter, as that the impressions may be equal. But, from thence we conclude, that such impressions are always as the quantities of matter and squares of the velocities conjointly; and consequently, when the quantities of matter are equal, the impressions will be as the squares of the velocities. The same experiment may be shewn perhaps in a more simple manner, by making pendulums of different quantities of matter, to strike with different velocities upon a smooth surface of soft clay; the velocities being always assigned by help of a graduated arch, as is done in the common experiments of the collision of bodies. In this manner,
likewise,

likewise, the impressions are found to be equal, when the products of the quantities of matter, by the squares of the velocities, are equal. Thus, let a hollow ball, with a quantity of matter 1, move with a velocity 20, and make a certain impression. If you afterwards inclose as much lead within the same ball, as to render it of double the mass of matter; you must give it a velocity somewhat exceeding 14, before it make an impression equal to the former one. If the body 2, receive only a velocity 10, the impression will be remarkably less. In the former method, the gravity or weight of the balls, continuing to act after they touch the clay, may contribute somewhat in making the impressions; but, in the other way of proceeding, the impressions must be more exactly owing to the *vis insita*, or force acquired by the bodies, in falling from the determined heights. This experiment can easily be reconciled with the old measure of forces, according to which the forces of equal bodies are to be estimated from the velocities simply. But, in order to do this, we ought carefully to distinguish betwixt two very different kinds of resistance, *viz.* that from the *inertia* of the

the matter, and the other from the force of cohesion. When an impression is made by a stroke, upon the surface of any yielding substance, such as soft clay; the resistance to be overcome in making that impression, proceeds almost entirely from the tenacity or cohesion: any resistance arising from the *inertia* of the small quantity of matter displaced in forming the pit or cavity, is quite inconsiderable. The force of cohesion or attraction amongst the several particles of any homogeneous body, is equal and constant; and therefore, must produce an uniform resistance to the motion of any body applied to overcome it, or a resistance proportional to the time of the action. The continued resistance of a few attracting particles in a slow motion, and the successive resistance of a greater number of particles in a quick motion, will equally diminish the force of a given spherical body, in an equal time. If then the force of any ball, moving with a certain velocity, be consumed by the resistance from cohesion in a given time; the force of the same ball moving with double velocity, will be consumed in a double time; if the velocity be triple, the time will likewise be triple; and the velocities

ties must always be proportional to the times in which they are destroyed. From whence it is manifest, that a ball striking with a velocity as 2, must make an impression fourfold of what it makes with a velocity as 1; because it continues likewise for double the time. In uniform motions, a double velocity and a double time, must always give a fourfold space: and the same thing must obtain in equably retarded motions; the spaces described being exactly the half of what would have been described by the uniform motions. But, tho' the impression be fourfold, the real effect, by which the force ought to be measured, is only double; for it has only undergone a double resistance. For the same reason, triple the velocity makes a ninefold impression. And, in general, the small impressions made upon the surface of soft clay, (or even upon marble) by bodies striking upon it, must, *caeteris paribus*, be, as the squares of the velocities. These impressions, made in opposition to the uniform resistance of cohesion, are no better measures of forces, than the heights to which bodies are thrown near the earth, in opposition to the uniform power of gravity. The heights

heights are known to be, as the squares of the velocities. When the velocities are, as 2 to 1, the heights are, as 4 to 1. But the forces are still to be estimated, as 2 to 1; since the times which the uniform action of gravity takes to destroy them, are in that proportion. With a double velocity, and in a double time, the body arrives at a fourfold height.

WHAT hath been here remarked, concerning the measure of forces, is not so much intended for the instruction of those who have had leisure to attend to this celebrated controversy, as to enable every one who has the smallest acquaintance with these matters, to judge for themselves; whether the common arguments, when rightly understood, be sufficient for the decision of the question? or, whether there be still occasion for a new solution of the difficulties? The debate has been closed long ago; after being managed by the ablest advocates on both sides: and the subject is generally thought to be exhausted, though no formal reconciliation hath hitherto been declared amongst the parties. We are now told, "That by a profusion of writing, the point is rendered so perplexed and intricate,

"tricate, that there appears not a better way
 "to come at the truth, than, neglecting the
 "arguments on both sides, to apply directly
 "to facts for a solution, as one would do up-
 "on a point newly started." With this pom-
 pous introduction, or preamble, is ushered
 in a new project for a treaty of peace amongst
 the *English, French, and German* philosophers.
 No mention is made of the *Italians, the*
Dutch, and others, tho' they were pretty
 deeply engaged in the quarrel. The reason
 for which omission may possibly be this, that
 the philosophers in *Italy, Holland* and else-
 where, chuse rather to preserve the appear-
 ance of neutrality, and act as mediators.
 A preliminary article being first settled, name-
 ly, to consider the several pretensions of the
 contending powers, as so many points newly
started, and that without any regard to their
 respective memorials; there arose a necessity
 in the next place, in order to render the ne-
 gotiation the more consistent with itself, for
starting new facts likewise, or at least of new-
 modelling the old ones. The method in
 which the whole affair hath been conduct-
 ed, and the jarring and opposite interests ad-
 justed, will best appear by an instance or
 two.

two. It is said, "That when a body is
 " thrown up with a double velocity, and
 " consequently with a double force; the rea-
 " son why it ascends four times its former
 " height, is plainly this, that the counter-
 " action of gravity, while it has a double
 " force to struggle with, has but half the
 " time, in any given space, to produce its ef-
 " fect." The natural meaning of which is,
 that if a body, thrown up with any velocity,
 rises to a certain height in a given time; a
 body thrown up with double the velocity,
 ought to ascend to an equal height in half
 that time. This indeed would hold true,
 if the upward motions could be supposed u-
 niform: but, as a body thrown up, happens
 always to be equably retarded, the real fact
 comes out to be very different. The body
 thrown up with the double velocity, rises to
 triple the height of the first body in an equal
 time, and to $\frac{7}{4}$ of that height in half the
 time. Let the height to which the first bo-
 dy rises in any time, be called 1 yard; the
 height to which the second body ascends in
 half that time, is 1 yard and $\frac{1}{4}$. The for-
 mer position however, as it was only taking
 a retarded motion for an uniform one, was
 pretty

pretty plausible, and good enough to pass upon *Germans* and other foreigners ; especially if they were strangers to the *English* language. Another allowable artifice to deceive the adversaries, is in endeavouring to make them believe, that “ all the world knows, that “ when bodies move through a fluid, or “ any soft matter, a double force produces a “ quadruple effect.” If the word *effect* were taken in its most proper sense, for the force communicated to the fluid or soft matter, by the moving body, this could never exceed the force of the body. A double force could never produce any more than a double effect in its own direction. All the world indeed may know, that when a body strikes upon clay or other such soft matter, a double force may produce a quadruple impression. But then, this can only happen in such cases, where no other resistance is worth the minding, but that arising from the cohesion of the matter, in the manner above explained. When a ball moves through a fluid, the resistance proceeds, almost entirely, from the density or *inertia* ; and, the density being given, this resistance must increase as the squares of the velocity. In this case, it would be a

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vain

vain attempt, to assign any measure of the impressions; because, it follows clearly from the demonstrations of Sir *Isaac Newton* *, that the body would go on for ever. And surely, it requires not much abstract reasoning to prove, that meer *inertia*, without the help of some active power, like gravity, or the attraction of cohesion, could never totally consume any motion, or reduce a body from a state of motion to a state of absolute rest. Yet, after all, as every one has heard, that the resistance of a perfect and incompressible fluid, was in the duplicate *ratio* of the velocities; this was foundation enough for a lover of peace, to assert roundly, that the impressions made in a fluid, were in that *ratio* likewise. Greater stretches might well be permitted for the accomplishment of so desirable an end, as a compleat union and harmony amongst philosophers.

WHEN one body strikes upon another with a great velocity, the parts immediately impelled, are sometimes broken off or driven away beyond the reach of attraction, before they have time to draw the other parts after them with any observable force; and the
force

* Princip. Lib. II. Prop. v. et cor.

force of cohesion, having opposed the impinging body for so short a time, makes no sensible resistance to it. The attraction of cohesion acting without interruption; any resistance made, or any motion communicated by it, must be gradually produced; and consequently, in a very small time, the effect will be proportionally small. Of this we have many examples. Let us suppose a board set nearly upright on its end, and so slightly supported, that a bullet thrown against it, out of a man's hand, will tumble it over; if the same bullet be discharged from a gun, it will go through the board without moving it out of its place. In the same way, a bullet has been known to go through a man's body, an arm has been taken off by a cannon ball, or even by the sail of a wind-mill, without any visible motion produced in the other parts of the body; though a much less force would be sufficient to drag the whole body forwards. The better to illustrate the manner in which these effects are performed; let it be observed, that by means of a small thread or a load-stone, a considerable mass of matter may be gently pulled alongst a table: whereas, if a sudden tug be given, or a greater force

force applied; the thread breaks, or the load-stone separates, without seeming at all to move the body. The nature of *inertia* and resistance has been so much misunderstood, or misrepresented, that it was necessary to explain these *phaenomena* from their true principles. In particular it might be thought, consistently with the false notions advanced concerning the resistance of matter, that the reason why a body did not move forwards when a part of it was broke off by a great force, was this, that the *inertia* or resistance to motion in that case, became stronger than the power of cohesion.

PHILOSOPHERS have fondly perplexed themselves, with many subtle questions concerning the communication of motion; and have persisted, with the utmost anxiety, in a very fruitless enquiry, how motion can pass out of one body into another: as if motion was something that could be separated from the moving body, and infused from one into the other, like water poured into a phial. But, notwithstanding all the intricacy of this affair, it would appear to be still a greater mystery, if one body in motion were not to move another lying freely at rest. It is indeed

deed only by experience that we learn the laws of the communication of motion. For who ever supposed, that either the general or particular properties of matter could be discovered in any other way? How could it be known, whether body was penetrable or impenetrable, divisible or indivisible, elastic or non-elastic, animated or inanimated, but by trial? Supposing however, that we were informed before-hand, that the body at rest was impenetrable, utterly inactive and soft, and had neither impediment nor tendency to motion; what would hinder us to foretell exactly every circumstance that happens, when another body of the same kind strikes upon it *?

WHEN a body in motion strikes upon another at rest; the one loses as much motion, as the other gains, in the same direction. This is ordinarily said to proceed from a reaction of the body that acquires the motion. The effect is indeed the same upon the percutient body; there is the same change of its state, as if it had actually received an impulse in the contrary direction. This, for the sake of shortness,

* Philos. Essays,

shortness, we may, and always do call, *the effect of reaction*; but we can never imagine that there is any real active opposition of the body at rest to the motion of the other. The great difficulty complained of, is in conceiving, how the one body has its motion diminished by the stroke as much as the other is increased, while there is no active repugnancy allowed in the one to the other. It would be ridiculous enough to fancy, that the motion of the one was a part of the motion of the other; and that the very same motion could be transferred from the one into the other. "As well might the magnitude, figure, or colour, of one body be imparted to another." But this we may venture to say, that, for ought appears to the contrary, it follows from the nature of a passive, extended and impenetrable substance, that motion is lost in one by producing it in another. It will easily be admitted, that one motion may destroy an opposite motion, and by that means lose of its own. Yet there is no less difficulty here, than in the communication of motion. If two pieces of clay opposing each other with equal forces, had any real activity of their own, it might be expected, that

that, tho' they both stopt at their first meeting, this was only done by suspending the effect of each other's action; and that they would resume their former degree of motion, upon being separated by a man's hand. We find however, that the motion of both bodies is irrecoverably lost by such a collision. If then the force of one body is lost or consumed by destroying force in another; why may not one body as well lose force by producing it in another. Besides, were there any real active opposition in the body at rest, to the body in motion; part of the force ought to be lost on that account, and the sum of the motions after the stroke, would be less than the motion before it.

WHEN a horse pulls a stone forwards, it is commonly said, that the stone pulls the horse back, with a force equal to that with which the horse pulls the stone forwards. But nothing more can possibly be meant, than that the horse loses as much force as the stone gains: and that, with respect to the horse, the effect is the same, as if there was no stone tied behind him, but that he was pushed back with a force equal to that acquired by the stone. The active force of the horse's
limbs

limbs presses forwards both the horse and the stone; what is called the reaction of the stone, is not equal to the whole force exerted by the horse; for then indeed there could be no progression: it is only equal to what is impressed upon the stone.

THE equality of action and reaction in the case of pressures, is to be understood in the same manner. When a finger is pressed against any body, the body is said to react upon the finger, as much as the finger is made to act upon the body. If the weight of the body be sustained by the pressure of the finger, there will be an active resistance from the power of gravity. And, if the figure of the body be violently kept in a bent or compressed state, there will be an active resistance from the cause of cohesion. But if the finger be applied so as to communicate an equal motion to all the parts of the body, and without opposing its gravity; there will be no occasion for imagining, that the body really reacts, or actively bears against the finger. The effect indeed upon the finger is the same, as if the body actually pressed upon it; but that effect can easily be accounted for, without having recourse to such a supposition.

position. The surface of a soft body, like that of the finger, pressed against a hard body, must undergo a change of figure, and receive the same impression, as if the hard body was pressed against it; because, when the parts that come first into contact with the body have their motion retarded, the other parts will advance farther forwards.

If there was any contradiction or absurdity in the common opinion of the communication of motion, this would be a sufficient reason for rejecting it, tho' it were ever so agreeable to the natural appearances of things. But shall we maintain, that every particular body can only be moved by a power of its own; and shall we deny, that motion is communicated by one body to another, meerly because we do not understand the precise manner in which motion is either conveyed or preserved? Why do we not, for the same reason, deny, that motion is begun at the command of any animated being? Or do we better conceive how any being or body moves itself, than how it moves another?

AN appeal has been offered to the common sense of mankind, as ready to declare in favour of the activity of matter. If the facts

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be fully represented, the public judgment need not be declined. Whatever obscurity the learned may find in this subject of the communication of motion, a vulgar observer will be very little perplexed about the matter. When a stone is thrown out of the hand, and continues to move when left to itself; there is no fear, but "every person, who has not "studied philosophy," will rather attribute this to a force some how impressed upon the stone, than to any action or operation of the lifeless stone. It will never enter into his head, that the stone flies away with wings of its own. Such a person, judging from first appearances that all motion languishes and decays, may, likely enough, be inclined to think that body rather affects a state of rest than a state of motion; and that the impressed force would not remain for ever: but he will never entertain the smallest doubt, but, that as long as the motion continues, it is only an effect of the first impulse. And, having once granted that motion can continue one moment as an effect after the action of the moving power is over, the same reason must remain for its continuance the next moment of time, and so on forever.

When

When we see a body continuing to move with the self-same velocity which is received from the hand, is it not more natural to say, that it does so for want of a power or activity to stop itself, or to destroy this motion, than to imagine the body to be no way determined by the impulse, but to be carried on with an equal force by a power of its own continually exerted after the impulse. Such obsequious complaisance of the stone, in imitating so exactly the motion of the hand, must appear extremely curious; especially if we consider what a violent opposition it is supposed to have made, before it yielded to the motion at first. It brings to remembrance the man in the parable, *who said, he would not go, but went*: or perhaps it may be likened to a young fellow learning to dance, who bends his limbs but awkwardly at first setting out, till being led about for a while in the master's hand, he can afterwards continue the step of himself. If, in the collision of bodies, the one body could thus adapt and conform itself in all cases to the motion of the other; it seems to be the highest injustice to refuse it the power of sensation and consciousness.

FROM

FROM experience and observation we learn, that body is equally indifferent to motion and rest. And this indifference appears to be the natural consequence of the most absolute inactivity. Body must exist either in the one state or the other. But, supposing it once existing in a moving state, the continuance of the motion implies no activity in the body, any more than it requires activity to preserve a quiescent body in a state of rest. And we may, with equal reason, enquire for the cause of the continuation of rest, as for a cause of the continuation of motion.

PHILOSOPHERS have indeed entertained various opinions concerning the continuation of motion. *Aristotle* endeavoured to explain it by means of a vehicle or the circumpulsion of fluid matter, hereby only evading the question, as the same difficulty returned, how the motion of this fluid continued. And many of the moderns have imagined rest to be the proper state of inactive matter, to which it naturally tended; and have ascribed the continuance of motion to the constant and immediate agency of the Deity, or of some delegated intelligent power.

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How far the concurrence of the Deity is necessary to the support of created beings in general, is perhaps impossible for us to determine: but, unless some continual influence of that sort be requisite, there appears to be no occasion for any special concurrence, in order to account for the continuance of motion. If however, it could be demonstrated, that body cannot continue to move on in a straight line, by virtue of the first impulse; what more rational solution will be found, than to have recourse to the efficiency of an intelligent principle? For tho' it should be granted, that the continuation of motion, and the communication of it from one body to another, cannot proceed from the *inertia* of matter; will it therefore follow, that every atom of unintelligent and undesigning matter is endued with an activity capable of moving it with the greatest order and regularity, as well as variety and diversity; conforming itself to the motion of other atoms with which it is connected; and adapting itself often to the will and intention of man? Why should we not rather rank this power of the preservation of motion, with those other active powers employed upon matter, tho' not essentially

entially belonging to it (such as gravity, attractions and repulsions of various kinds, &c.) and refer them all to an intelligent cause, if there be herein discovered the same signatures of thought and design?

THIS leads us to another source of objections against the inactivity of matter. As the very resistance which matter makes to an external force, and its preservation of the motion received, have been taken for symptoms of activity; we need wonder the less to find an activity suspected in matter from its gravitation, and these other instances where motion begins in bodies, without any visible cause.

It may be objected, "That a stone falls
"to the ground without any external impulse, so far as we can discover; and therefore dead matter begins motion of itself." Such manner of reasoning would make short work of natural philosophy. Because there are a variety of motions, changes and transformations, produced every day amongst inanimate bodies; is it straight way to be concluded, that these bodies move themselves? The contrary of this appears in so many instances, as gives good reason to believe it never

ver happens in any case. We see for certain, many motions begun by animated beings; we observe many bodies moved by the impulse of other bodies; and the mechanical causes of some motions, have through time been discovered, which were not formerly perceived. Once in a day, it was found philosophy to maintain that smoke and vapour mounted upwards of themselves by a principle of levity, tho' now one might as well assert that cork rises up of itself in water. Many *phænomena* were explained from an abhorrence of a void, which was fully as reasonable a passion in a dead body, as the love of a center, or an inclination to meet with other bodies. The rise of water in pumps was ascribed to a self-moving power in the water, and the pulsation of the arteries to a power they had of dilating themselves. The power of magnetism bears some resemblance to that of gravity; the theory of it is still imperfect: yet there are a multitude of facts which indicate a mechanism by means of some effluvia; particularly the new method of making artificial magnets by attrition, and the well known experiment, in which an iron rod is presented to a magnetic needle, the
same

same end being made to attract, sometimes one pole and sometimes another, by only changing the position of the rod, or inverting it up side down; and much more by striking upon it with a hammer, or beating it against the ground. Electricity is another more palpable instance of the same sort. That a fluid is concerned in producing the apparent attractions and repulsions, and other more astonishing effects of the electric globe, can hardly be doubted, however difficult it may still be to describe the laws to which it is subjected in its operations. Why then should it be accounted "whimsical" or unphilosophical to demand a cause for the attractive power of gravity? Tho' all the mechanical accounts, hitherto given of the cause of gravity, should be found unsatisfactory; may it not still be owing to some unknown mechanism, or the intervention of matter, moving other matter? Or, tho' it were shown to be impracticable by any mechanism whatever, as is not improbably the case; why may we not attribute it to the immediate agency of an intelligent active being?

It may be urged further by way of objection, "That a power of beginning visible
" motion

“ motion is no more connected with a power
 “ of thinking, than it is with any other pro-
 “ perty of matter or spirit.” This may
 possibly be admitted in a certain sense, *viz.*
 that there may, for ought we know, exist
 some species of thinking beings, destitute of
 the power of motion altogether. Oysters
 have very little of it. But however this be, we
 know, with all the certainty attainable in
 physics, that many thinking beings have
 such a power ; we see them begin motion, a
 relative motion on the ground. When that
 motion is lost, they renew it and vary it again
 at every step. They not only begin new
 motion, but destroy old motion, at pleasure :
 whereas no experience can ever tell us, that
 the beginning of the visible motions of dead
 matter is original and underived. And there
 is this wide difference (which hath been often
 remarked by authors on this subject) betwixt
 animated and inanimated beings, with respect
 to motion, namely, that the thinking being
 can determine the direction and quantity of
 its motion : which is a power incompatible
 with dead matter ; and consequently it will
 of itself remain for ever incapable of the least
 motion. Thus far therefore there is a con-

P.

nexion

nexion betwixt motion and thinking, that a power of beginning motion seems necessarily to infer a power of thinking; tho' we cannot affirm inverfely, that a power of thinking must infer a power of beginning motion.

BUT if the bare beginning of motion seems to require an intelligent caufe, the power of gravity, furely, has the higheft title to lay claim to that origin. The motions arifing from gravity are evidently of fuch a fort, as cannot, without the greateft violence to reason, be afcribed to any blind tendency betwixt the attracting bodies. This will beft appear upon ftating fome of its known effects. A ftone is drawn towards the earth in *Europe* and in *America*; it changes its direction in different places, pointing always nearly to the center of the earth, (or exactly in a line perpendicular to the level furface) in the fame manner as iron does towards the loadftone, or a feather to the electric tube. The attractive force of a ftone diminifhes, the farther it is removed from the earth, according to a fixed rule, or as the fquare of the diftance encreafes. A body placed by itfelf would move no way; but two bodies run together. A given body is more attracted to a large quantity

quantity of matter than to a lesser. Is it then conceivable, *that* an unthinking being should be endued with an activity which it regulates and varies in proportion to the situation, distance, and magnitude of another body, whilst it is supposed to be not in the least influenced or acted upon by that other body, or any other being whatsoever? If this shall be maintained, another question will arise. By what actions, or what stronger language than this, can any man convince his neighbour of his own reason or understanding? It is presumed, that a higher degree of evidence will hardly be required in physical matters, than what we have for the life and existence of one another: the voice of nature as loudly declares the origin of gravity, that ruling principle which binds the parts of the system together. From the circumstances observable in the apparent mutual tendency of bodies, we are naturally led to conclude, that gravitation is the effect of the continued and regular operation of some other being upon matter; and that bodies are either drawn or pressed together by something external. A power so constant, so regular, and withal so uniformly varied and diversified

fied according to different circumstances, can proceed from nothing but an intelligent cause, either mediately or immediately exerted upon bodies.

WHEN motion is observed to begin in any body, it must be ascribed to one or other of these four causes; 1. Some external animated being; 2. An external inanimated being; 3. A self-moving animated faculty; and 4. A self-moving inanimate faculty. Of the former three, there are a variety of manifest examples. Of the last there can be no certain example at all; and consequently we should make a bad choice, in preferring it to the others: such a supposition, namely the production of motion from an internal inanimate principle, is intirely without foundation; and seems to be much the same thing as to allow, that motion may begin without any cause at all.

THAT something may begin, to exist, or start into being without a cause, hath indeed been advanced in a very ingenious and profound

found system of the sceptical philosophy*; but hath not yet been adopted by any of the societies for improvement of natural knowledge. Such sublime conceptions are far above the reach of an ordinary genius; and could not have entered into the head of the greatest physiologist on earth. The man who believes that a perception may subsist without a perceiving mind or a perceiver, may well comprehend, that an action may be performed without any agent, or a thing produced without any cause of the production. And the author of this new and wonderful doctrine informs the world, that, when he looked into his own mind, he could discover nothing but a series of fleeting perceptions; and that from thence he concluded, that he himself was nothing but a bundle of such perceptions.

Mr. Baxter, in his *Inquiry into the Nature of the Human soul*, and likewise in his *Maths*, endeavours to prove, that gravity cannot be

* *Treatise on Human Nature*, 3. vols. octavo. This is the system at large, a work suited only to the comprehension of Adepts. An excellent compend or summary whereof, for the benefit of vulgar capacities, we of this nation enjoy in the *Philosophical Essays*, and the *Essays Moral and Political*. And to these may be added, as a farther help, that useful commentary, the *Essays on Morality and natural Religion*.

be a property inherent in matter ; from this consideration, that, if body had an actual tendency to fall down, it could not at the same time resist the downward motion by its *inertia*. Which reasoning plainly supposes, that there is a real active resistance arising from *inertia*. And indeed, tho' he expressly intends to deny the smallest activity in matter, yet he everywhere speaks of *inertia*, as a *conatus* or tendency essential to matter, by which it makes a violent opposition to the power of gravity, and other active powers. This being a question of the utmost importance, it may not be amiss to offer an amendment upon that argument, which will free it from any inconsistency, and place it on its true bottom, on which alone it can stand its ground. To prove then that gravity does not proceed from any internal active force of any kind, either in the earth or such bodies as fall towards it ; it is only necessary to assume one axiom, that no substance can actively tend to move to two opposite sides, at the same time. It is easy to conceive that a body may be pushed or drawn to opposite sides, by two contrary external forces : in which case, if they are equal, the body will remain at rest ; if unequal, it will be moved

ved to one side, by the difference of the two forces. Thus, every part of a stagnating fluid is pressed equally in every direction. But if a body actively tend to one side, it cannot actively tend to the other at the same time; that being a contradiction in terms. And in fact, we shall find it impossible, to make two such efforts in our own bodies, to move them *wholly* to two opposite sides. This being granted, it follows, that if the waters of the sea gravitate towards the earth by an active force, they cannot, at the same time, gravitate by their own activity towards the moon, so as to produce the tides. And therefore, the gravitation in one or both cases, is owing to an external influence. The same argument, if it shall be found conclusive, may be applied to the attraction of the moon towards the earth and sun, in the conjunctions, and other such like cases. The attractions betwixt small corpuscles, will be shown to proceed from an external cause, in the same manner. And hence it will follow, that elasticity and the chymical effervescencies, are not the result of the activity of attracting particles. And for the same reason, a body thrown upwards, cannot move up by an

an active force of its own ; for that would be opposite to the active force of gravitation.

If, in order to evade the foregoing argument, it should be said, that a body never tends to two opposite bodies at the same time; but tends only to one side with the difference of the forces with which it would tend to each of these bodies taken separately: this will at least afford another remarkable instance of that exact regularity with which the power of gravity is varied and adapted to the different situations of bodies ; and which it is quite inconceivable that blind matter should perform without the superintendence of an intelligent being.

It has indeed been frequently asserted, that bodies move towards each other by virtue of a law originally established. But law, that is to say, a mere abstract name or complex notion, which is no real being, cannot impel a stone, and cause it to begin to move. Law by itself, with submission be it spoken, will avail nothing, unless either the subjects of it have understanding to yield a willing obedience, or they be compelled to it by external force.

IF the first appearances of things are to be trusted, there is full as good reason to assert, that the earth *draws* the stone, or the magnet the iron, as that the stone or iron move of their own accord. Yet the supposition of one body drawing another body at a distance, without the intervention of other matter, is universally rejected; and that merely because of the natural impossibility of the thing. That a being cannot act where it is not, any more than when it is not, is an axiom or principle of reason and common sense, and not a lesson of experience. And is it not equally self-evident, that dead matter can never begin motion of itself; far less regulate its motions according to a law? If bodies are not sensible of the neighbourhood of other bodies, of their quantities of matter, and of their precise distance from them; is it to be imagined that they will move themselves with such determined degrees of force, corresponding to the different quantities of matter and different distances?

THE active powers both of attraction and repulsion are of such a sort, as could not be exercised by the bodies themselves, without either distinct perceptions of their own situations

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and

and magnitudes with respect to other bodies, or a regular succession of some kind of clear perceptions, corresponding to every variety of situation and magnitude, and all this accompanied with a memory and a power of comparing past with present perceptions. It would therefore, make nothing to the present purpose to have recourse to that extraordinary subterfuge, made use of by Mr. *Hobbs* on a like occasion, That all matter is endued with an obscure sense and perception, and wants only the organs and memory of animals. And it hath never yet been alledged, that all matter is essentially endued with a clear and distinct sensation or consciousness. If it should, we need be at no loss for an answer. For, were that the case, why have we no perception of this activity that is supposed to be exerted by our own bodies in falling to the ground?

Dr. *Clark*, *Wollaston*, and others, have so fully proved that matter is incapable of any degree of thinking, that it is impossible to confute their arguments, but by scornfully denying the force of all metaphysical demonstrations whatever. If matter thinks, then either thinking is essential to all matter, or it arises from the modification, magnitude, figure

gure or motion of certain parcels of matter. But what can be more ridiculous than to imagine, that matter is as essentially conscious, as it is extended ! Will it not follow from that supposition, that every piece of matter, being made up of endlessly separable parts, (that is, of parts which are as really distinct beings, notwithstanding their contiguity, as if they had been at the greatest distance one from another) is made up also of innumerable consciousnesses and infinite confusion ? And farther, if every part of matter be self-conscious, it would be a contradiction to suppose that any system could be so. The resulting sensation or consciousness at last being but one distinct sensation or consciousness, (as is that of a man) the sensation or consciousness of every one of the constituent particles, would be the individual sensation or consciousness of all and each of the rest. In the next place, the faculty of thinking cannot arise from the size, figure, texture, or motion of body ; nor can be destroyed by any alteration of these qualities : because bodies, by any change of these, only become greater or less, round or square, rare or dense, translated from one place to another,

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with this or that new direction, or velocity ; or the like. All which ideas are quite different from that of thinking ; there can be no relation between them.

THIS sort of reasoning deserves at least some direct answer, and is not to be overthrown by any criticism on the meaning of the word *immateriality*. This, as is learnedly observed, most certainly “ comes out to “ be merely a negative term, comprehending “ every thing that is not matter.” And it is not to be doubted that if other terms, such as *immortality* and *infinity*, were but examined with the same “ sufficient accuracy,” the one might come out to be merely a negative term, comprehending every thing that is not mortal, and the other a negative term comprehending every thing that is not finite. Such premisses as these are undeniably true ; the only difficulty lies in discerning the use and importance of them. But if the reasoning subjoined to these premisses “ be found “ entirely conclusive,” the consequence must be, that, in judging of the qualities of matter, we are in every case to rely upon the report of our external senses, and never to employ our reason in comparing one thing with

with another, in order to correct our first impressions. *Epicurus* is to be applauded for believing on the credit of his eye-sight, that the sun and moon were no bigger than a cart-wheel or a cheese. The peasant, who thinks that the sun moves from east to west every day, is wiser in that respect, than the greatest philosopher; and *Copernicus* is to be regarded as a common enemy to mankind, "for declaring war against our senses."

THE grand question to be resolved in this controversy, is not whether we have clearer conceptions of material or immaterial substances, as we are equally ignorant of both; but whether the power of thinking, which is a known faculty of some substance, be not absolutely indivisible? and as such, incompatible with the known properties of matter? The most plausible objection that can well be made against this argument, arises from the late observations made concerning the polypus, and other living creatures of that kind. Yet these, when duly considered, will be found entirely consistent with the indivisibility and unity of thinking substance. They do indeed exhibit a remarkable peculiarity in the manner of the propagation of these animals;

animals ; and perhaps give some ground for conjecturing, that a number of animals, or a system of thinking beings, may probably be conjoined together, under the form of one animal. We were formerly acquainted with instances of two or more animals connected or adhering together, as in worms of different kinds, tho' they do not grow or extend themselves to any larger dimensions upon separation. Other animals were known, which, upon losing a limb, had it regenerated again, as is the case of craw-fish and lobsters. May it not then be supposed, that, in the polypus, both these cases concur together ?

He who allows, that the system of the universe may proceed at present without any guidance or direction, will find it difficult to assign to himself any good reason, why it might not always, or from eternity, have done the same. The excellency and perfection of the material world, can be no evidence to him of an original architect or contriver of all things : since the highest excellencies and perfections are acknowledged, even by every Theist, to exist without a cause. But the instances of a wise administration and superintendence, presented every moment to
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our view, strike the mind in the most forcible manner. We are eye-witnesses of providence, but not of creation. The contemplation of every part of nature, furnishes us with irresistible proofs of intelligence, counsel and design, still employed in actuating, moving, conducting and governing the universe. *Nihil est enim, quod ratione, et numero moveri possit sine consilio, in quo nihil est temerarium, nihil varium, nihil fortuitum. Ordo autem siderum et constantia, neque naturam significat; est enim plena rationis: neque fortunam, quae amica varietati constantiam respuit. Quae qui videat, non impiè solum, verum etiam indoctè faciat, si Deos esse neget. Nec sane multum interest, utrum id neget, an eos omni procuratione, atque actione privet: mihi enim, qui nihil agit, esse omnino non videtur.* Cic. de Nat. Deor. lib. II. cap. 16.

WE observe indeed, in various instances, a subordination of natural causes and effects, and a dependence of one thing upon another. To investigate these, is the proper province of natural philosophy. The philosopher's business is, to learn the constitution of things as they really are; and to search out the laws and
order

order established in the material world, and by which it is conducted. The most successful enquirers into nature, have seen most reason to resolve all things finally, into an incorporeal, intelligent, and powerful first cause: and have rejoiced in the persuasion, that they themselves, and all parts of the universe, are the offspring, and under the administration, of the same great, wise, and beneficent parent. But they are not solicitous in limiting the number of second causes, far less in excluding them altogether, as some over zealous friends to religion have done. *Plutarch* *, in recommending the philosophy of *Anaxagoras*, observes, that superstition proceeds from ignorance of causes, and want of experience; but that natural philosophy banishes all superstition, and begets in the mind a well grounded piety, with comfortable hopes.

MANY

* Δεισιδαιμονία προς τα μετεωρα θαμβος εργαζεται ποις αυτων τε τουτων τας αιτιας αγνοουσι. ο φυσικος λογος την ασφαλη μετ' ελπιδων αγαθων ευσεβειαν ενεργαζεται.

VII. PERICL.

MANY fruitless attempts have formerly been made to explain all the *phaenomena* of nature, on mechanical principles alone. But it is exceedingly surprising, that, in the present age, so eminent an astronomer as M. *Mau-pertuis* * should again revive exploded notions; and suggest, that the planets may possibly be retained in their orbits, by the motion of a circumambient fluid, as an ultimate cause. There is nothing more demonstrably certain, than that gravity cannot arise from the pressure of a whirlpool, or vortex of a dense fluid, once put in motion, and continuing to move round of itself, as M. *Des Cartes* imagined. And it is needless at present to examine, whether it can be better explained by means of a rare elastic aether. If Sir *Isaac Newton* endeavours in that manner to account for gravity, the attraction of cohesion, the reflexion and refraction of light, &c. it is only with a view to point out some more general mechanical cause, upon which all these powers may possibly depend. He never could believe that his aether was possessed of a real activity of its own. The elasticity of that

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fluid

* *Cosmologie.*

fluid, must itself either proceed from some higher mechanical cause, or flow immediately from some vital intelligent principle, which must be immechanical. The mutual repulsions betwixt one particle and another of the fluid, and betwixt the fluid and the bodies swimming in it, discover as much variety and regularity, as were before observed in the *phaenomena* of gravity; and must consequently be regarded as the continual effects of thought and design.

It seems to have been far from Sir *Isaac's* intention, to ascribe activity to matter in any shape; tho' his meaning has been sometimes mistaken *. To do so, would be a manifest contradiction to the primary laws of motion, delivered by himself in the beginning of his *Principia*.

ACCORDING to *Spinoza*, one piece of matter is moved by another, and that by a third; and thus there is a progression of causes and effects, *in infinitum*, all acting blindly, without intelligence and design. Not to repeat here, the metaphysical argument against the possibility of an endless series of dependent causes

* *Philosophical Essays*. p. 119.

causes of any kind, it will be a sufficient confutation of this scheme to observe, that it supposes that an infinite number of undesigning beings can continually produce the greatest order, regularity and harmony: which is no better sense than to say, that, tho' one single cypher be of no value, yet an infinite number of nothings can amount to a real quantity. There is no other possible method whereby to judge of the intelligence or wisdom of any being, but by it's sensible effects; and we may as easily suppose one single lifeless being to produce by itself the ordinary effects of wisdom, as that they can result from the successive action of an infinite number of them.

BECAUSE the world is often compared to a machine, it hath been imagined, that, when once set a going, it may continue its motions without any further interposition of the Author of nature, or any other being. If we attend however to any machines of human contrivance, we only learn, that all their motions depend upon certain active powers, as gravity and elasticity. These are employed by ingenious artists, such as they find them, to many useful purposes in life. But it is by
considering

considering the effects of these powers in general, and without regard to any particular application of them, that we must determine concerning their nature. And it hath been already attempted to shew, that they are all probably derived from an active intelligence, either immediately or mediately exercised upon matter. A connection manifestly appears amongst different parts of the system of the world; and many bodies in it, tho' dead and inanimated, are not only moved and agitated themselves, but communicate motion to each other, according to a certain fixed and established order, which is called the course of nature. But there is no foundation in mechanics for imagining, that a lifeless body, in consequence of a law promulgated some thousands of years ago, can move itself with as much regularity and variety, as if it was actually endued with thought and reflexion.

THIS, to most people, will appear an impossibility, as much as to make a blind man to see, whilst he continues blind. And there are few, but will at least acknowledge the contrary opinion to be supported with so high a degree of probability, as justly to deserve

serve the preference. *Haec quidem, si non vera, saltem verisimillima videtur.*

“It is alledged, that the system of the
 “world would discover more contrivance, if
 “it went on of itself, without any concur-
 “rence of the Deity.” That Mr. Boyle
 gives any countenance to this opinion, cannot
 be positively concluded from his words. He
 does not say, that brute matter moves itself,
 but that “it is managed by certain laws, and
 “upheld by God’s ordinary and general con-
 “course.” The less power is exerted to pro-
 duce a given effect, the mechanism may just-
 ly indeed be esteemed the more perfect. But
 a machine going on without a moving power
 at all, is a thing quite unheard of. And
 where would be the beauty of every body,
 every wheel moving itself, without a depend-
 ence of one part upon another? There would
 then be no mechanism at all. Let the ma-
 chine be as grand and perfect as possible; it
 must stand in need of a first mover, not on-
 ly to begin the motion, but to preserve it. If
 every part spontaneously moved itself, it is
 then no machine; some other name must
 be found for such an arrangement or system
 of things. If all the motions and changes
 of

of bodies are performed immediately by these bodies themselves, without the influence of other matter, or any other power; there is an end of all enquiries into causes and effects; philosophy must be degraded to a bare knowledge of facts, a history of nature.

THERE is an argument, in the form of a *reductio ad absurdum*, brought against the received opinion of the equality of action and reaction, which had almost been overlooked. The substance of it is this, that as the pressure *undequaque*, is a consequence of the law of equal action and reaction in fluids; so the same sort of pressure ought to be a property of solid bodies likewise, if the same law be universally extended to all bodies, solids as well as fluids. For a ready answer to this objection, we need only recollect the definition of a fluid, as distinguished from a solid body. An *undequaque* pressure cannot obtain in a solid body, unless the particles of it did yield to any force exercised upon them; and, in yielding, were easily put in motion amongst themselves; or in other words, unless the solid body was converted into a fluid. A parcel of dry sand may bear some small resemblance to a fluid; its particles may be moved

ved more easily amongst one another, than these of a firm body, whose parts are closely united by attraction : yet still there must be a considerable adhesion of the particles of sand, on account of their irregular figures, and the inequalities on their surfaces. When a man applies his hand to a hole in the side of a vessel of water, he sustains a pressure from the tendency of the particles to slide down and make their escape : but, when his hand is laid on the side of a sand bank, there is no such pressure to be felt ; because the particles are intangled together, and support one another. Thus a body laid upon a rough inclined plain, may remain fixt, by the resistance from attrition alone ; whereas, if the plain be smooth, some other power becomes necessary to hinder the descent of the body.

THE mechanical action and reaction of matter, or the changes with regard to motion and rest, which take place in the collisions of non-elastic bodies, have been already considered ; and were found to be entirely consistent with the most absolute *inertia*. But that real actions and equal opposite reactions, obtain in the active powers of attraction and repulsion,

repulsion, is agreeable to perpetual experience; and is no less certain, than that these powers themselves are established in nature. We find the loadstone attracts iron, and that iron attracts the loadstone, with equal force; and, because they attract each other equally, they remain at rest when they come into contact. If a mountain, by its gravity, pressed upon the earth, and the earth did not react equally on the mountain; then the mountain would necessarily carry the earth before it, by its pressure, with a motion accelerated *in infinitum*. The same is to be said of a stone, or the least part of the earth, as well as of a mountain. A mutual attraction too is observed amongst the distant bodies of the solar system.

WHEN a heavy body lies upon a table, and consequently acts upon it with its whole weight; there must be an equal reaction from the power of cohesion of the parts of the table, in order to support the burden. In the same manner, if the bottom of any vessel sustains the weight of a certain quantity of sand, it must react with a force equal to that weight; otherways it must break or give way. The power of cohesion may indeed greatly
exceed

exceed the particular weight which it happens to counteract at any time : and the same table, at different times, may support very different degrees of weight. But still it is true, that the force exerted by this power upon the body, can neither be greater nor less than its weight, since the reaction barely supports the body without beginning motion in it. If this needs any illustration, we may suppose a man to have a weight hanging at his hand, while at the same time he presses that hand upwards against any fix'd body ; the force with which the hand adheres to, or is pressed against the body, may be greater or less at pleasure ; but the force employed against the weight, or that which directly opposes and counteracts it, must exactly be equal to the weight itself.

THE argument against the law of action and reaction may be made to conclude with equal force against the law of gravity itself ; and, if it proves any thing at all, will even prove it impossible that all terrestrial bodies should be endued with weight. *Mutatis mutandis*, the reasoning proceeds thus. Admitting the law of gravity, a smooth level surface must be the consequence. And as the law is not con-

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finied to fluids, but is supposed to be an universal law of matter; a smooth level surface must not only be the consequence in fluids, but in solids, equally. Here then is a fair *dilemma*. We must either acknowledge a smooth level surface to be always found in sand, powder, and indeed in all loose bodies, as well as in fluids; or confine this law of gravity to fluids. Can we remain a moment in doubt betwixt these two opposites? we are certain that a smooth level surface is not a necessary property of solids. The gravity of a whole hill of sand, is but an assertion without evidence. What remains then, but that we adhere to the former, and reject the latter, except as to fluids? And thus our author luckily, tho' without intention, has furnished a very convincing argument against the universality of this supposed law of gravitation. If a smooth level surface is an effect of this law, it follows clearly, that this law takes not place in solids, at least not universally, as in fluids.

It hath been judged a subject worthy "to be regreted that natural philosophers and mathematicians are not always well skilled in logics." A fair comparison alone can show,

show, whether they be more obnoxious to this censure than other people, and who are the most guilty of fallacious reasoning, and in particular of that species of it commonly called *Ignorantia Elenchi* or *ἰρροζήσις*. It must indeed be avowed, that few of the mathematical philosophers have testified any high admiration of those spacious openings and enlargements lately struck out by certain bold and enterprizing undertakers in the dialectic art. Nor can it reasonably be expected that they should entertain the most favourable opinion of such performances. Men who puzzle themselves with self-evident axioms, and stumble at the plainest demonstrations, raise a shrewd suspicion that they may be liable to like human infirmities in other matters, and can have no pretensions to be received as infallible guides. The farthest that complaisance can go, is to transfer the compliment, and to regret, that these *universal philosophers* are not always well skilled in the elements of mathematics and natural philosophy. * If their end in view be really
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* Mess. *Hobbs*, *Toland* and *Collins*, have made little other use of the mathematical philosophy, than as a touch-stone for discovering their own metal. Witness *Elementa Physicæ*, *Letters to Serena*, and *Reflexions on Mr. Clarke's second defence*. p. 36

the investigation of truth, as it is to be wished; a little more conversation and familiarity with *Euclid* and other geometricians, might be of good service to them, by accustoming their minds to the steady pursuit of real knowledge: but if their highest aim in life be vain disputation, and an ostentatious display of their abilities, in attempting to involve the clearest truths in doubt and uncertainty; better were it for them to throw away the rule and compass altogether, and to exercise their faculties on other subjects, where there may be more room for subtle evasions, and where mistakes, tho' equally remote from truth, and perhaps of more pernicious consequence to mankind, cannot, from the nature of the thing, be so easily detected.

ART.

ART. III,

Pappi Alexandrini collectionum mathematicarum libri quarti propositio quarta generalior facta, cui propositiones aliquot eodem spectantes adjiciuntur; auctore MATTHEO STEWART, in Academia Edinensi Matheseos Professore.

PROP. IV. Lib. 4. Collect. Math.
Pappi Alexandrin.

Sit [Tab. 1 Fig. 1.] circulus ABC, cujus centrum E, diameter BC, et recta linea contingens AD, quae cum BC in puncto D conveniat. Ducatur autem DF, et juncta AE, producat ad G, et FKG et GLH jungantur. Dico KE ipsi EL aequalem esse,

Factum jam sit, et ipsi KL parallela ducatur HXM. Ergo MX est aequalis XH; ducatur etiam a puncto E ad FH perpendicularis EN:

EN : aequalis igitur est FN ipsi NH : erat autem et MX aequalis XH : ergo NX ipsi FM est parallela ; et angulus HNX aequalis est angulo NFM, hoc est, angulo HAX, et in circulo sunt puncta A, N, X, H ; est igitur angulus ANH aequalis angulo AXH, videlicet angulo AEL : et propterea in circulo sunt puncta A, E, N, D ; rectus est enim uterque angulorum EAD, END.

Componetur autem sic. Quoniam uterque angulorum EAD, END est rectus, puncta A, D, E, N in circulo erunt. Aequalis igitur est angulus AND angulo AED. Sed angulus AED est aequalis angulo AXH, propterea quod parallelae sunt ED, XH : ergo in circulo sunt A, N, X, H puncta : et angulus HAX angulo HNX est aequalis : angulus autem HAX aequalis est angulo HFM : ergo FM ipsi NX est parallela ; et est FN aequalis NH : quare et MX ipsi XH aequalis erit ; estque ut XG ad GE, et ita XM ad EK, et HX ad LE : ut igitur XM ad EK, ita HX ad LE : et, permutando, aequalis autem est MX ipsi XH : ergo et KE ipsi EL est aequalis.

Perpendenti autem hanc propositionem, ejusque pulcherrimam resolutionem et compositionem statim mibi

mibi occurrit veram esse, etsi recta BC non sit diameter; si vero bisecta sit in puncto E, et iisdem fere verbis quibus utitur Pappus ostenditur, ope sequentis Lemmatis.

LEMMA. *Tab. 1. Fig. 2. 3.*

Sint duae rectae AB, CD circulo inscriptae, sibi mutuo occurrentes in puncto E extra circulum, et bifariam secentur rectae AB, CD in F, G punctis, a puncto E ducatur EH circulum contingens in H, erunt puncta E, F, G, H in circulo.

Et, iisdem manentibus, si sit punctum H in circumferentia, atque puncta E, F, G, H in circulo, continget juncta EH circulum in H.

CAS. I. Si una [Fig. 2.] rectarum AB, CD, puta recta AB, sit diameter; jungantur FG, FH.

Quoniam recta CD bisecta est in G, et est F centrum circuli quoniam diameter AB bisecta est in F; rectus erit angulus FGE; rectus

rectus autem est angulus FHE, quoniam contingit EH circulum in H, quare erit angulus FGE aequalis angulo FHE: in circulo igitur sunt puncta E, F, G, H.

Et, iisdem manentibus, si sit punctum H in circumferentia, atque puncta E, F, G, H in circulo, continget juncta EH circulum in H.

Quoniam AB diameter bisecta est in F, erit F centrum circuli; et quoniam recta CD bisecta est in G, rectus erit angulus FGE; est autem angulus FHE aequalis angulo FGE, quoniam in circulo sunt puncta E, F, G, H; rectus igitur est angulus FHE; quare continget EH circulum in puncto H.

CAS. 2. Si neutra [Fig. 3.] rectarum AB, CD sit diameter, sit K centrum circuli, et jungantur KF, KG, KH, FH, GH et KE.

Quoniam rectae AB, CD bisectae sunt in F, G punctis, erunt anguli KFE, KGE recti; et quoniam contingit EH circulum in H, rectus erit angulus EHK. Quoniam recti sunt anguli KGE, KHE in circulo sunt puncta E, K, G, H; quare erit angulus EGH aequalis angulo EKH; et quoniam recti sunt anguli EFK, EHK in circulo sunt puncta E, F, K, H; quare erit angulus EFH aequalis angulo

gulo EKH, hoc est, angulo EGH : in circulo igitur sunt puncta E, F, G, H.

ET, iisdem manentibus, si sit punctum H in circumferentia, atque puncta E, F, G, H in circulo, continget juncta EH circulum in H.

QUONIAM rectae AB, CD bisectae sunt in F, G, et est K centrum circuli, erunt anguli EFK, EGK recti ; quare in circulo sunt puncta E, F, G, K ; in circulo igitur sunt puncta E, F, K, G, H ; quare erit angulus EHK aequalis angulo EGK ; rectus autem est angulus EGK ; rectus igitur est angulus EHK ; quare contingit EH circulum in H.

Propositio Pappi aliter enunciari potest hoc modo.

PROP. Tab. I. Fig. I.

Sit circulus ABC et recta BC circulo occurrens in B, C; sitque recta AD circulum contingens in A, et rectae BC occurrens in D; bifariam secetur BC in E, et occurrat juncta AE
T circulo

circulo in G; per punctum D ducatur quaevis recta circulo occurrens in F, H; et GF, GH, jungantur rectae BC occurrentes in K, L; erit EK aequalis ipsi EL.

Factum jam fit, et ipsi KL parallela ducatur HM rectis GF, GA occurrens in M, X; et bifariam secetur FH in N, et jungantur NE, NX, NA et AH.

Quoniam est EK aequalis ipsi EL, erit et MX aequalis ipsi XH; est autem FN aequalis ipsi NH; parallelae igitur sunt rectae NX, FM; quare erit angulus HNX aequalis angulo HFM, hoc est, angulo HAX; in circulo igitur sunt puncta A, N, X, H: est igitur angulus ANH aequalis angulo AXH, hoc est, angulo AEL; quare in circulo sunt puncta A, E, N, D; contingit igitur AD circum [per *Lem.* praec.] in A, quod quidem ita se habet.

Componetur autem sic. Quoniam contingit DA circum in A, in circulo sunt puncta A, E, N, D [per *Lem.* praec.]; quare erit angulus AND aequalis angulo AED, hoc est angulo AXH; in circulo igitur sunt puncta
cta

cta A, N, X, H; quare erit angulus HNX
aequalis angulo HAX, hoc est, angulo HFM;
parallelae igitur sunt rectae NX, FM; et quo-
niam est FN equalis ipsi NH, erit et MX ae-
qualis ipsi XH, quare erit et EK aequalis ipsi
EL. Q. E. D.

*Alias quoque propositiones huic spectantes ex-
cogitavi, quarum aliquot huic subijcere visum
est.*

PROP. I. Tab. I. Fig. 4.

Sit circulus ABC, et recta AB circulo
occurrentes in A, B; et sit C punctum
in circumferentia circuli; jungan-
tur CA, CB, sitque recta DE paral-
lela rectae AC, rectis AB, CB oc-
currentes in D, E; per punctum D
ducatur quaevis recta circulo oc-
currentes in F, G; et CF, CG jun-
gantur rectae DE occurrentes in
H, K; erit rectangulum HEK ae-
quale rectangulo CEB.

Factum

FACTUM jam fit, et jungantur BH, BF. Quoniam est rectangulum HEK aequale rectangulo CEB, in circulo sunt puncta K, H, C, B; quare erit angulus BHD aequalis angulo BCG, hoc est, angulo BFD: in circulo igitur sunt puncta B, H, F, D; aequalis igitur est angulus ADH angulo CFB, hoc est, angulo CAB; quare erit DE parallela rectae AC. Quod quidem ita se habet.

COMPONETUR autem sic. Quoniam est recta DE parallela rectae AC, erit angulus ADH aequalis angulo CAB, hoc est, angulo CFB: in circulo igitur sunt puncta B, H, F, D; quare erit angulus BHD aequalis angulo BFD, hoc est, angulo BCG: in circulo igitur sunt puncta K, H, C, B; quare erit rectangulum HEK aequale rectangulo CEB. Q. E. D.

LEMMA.

Sint rectae A, B, C, D; E, F, G, H; sitque ut A ad B, ita C ad D, et ut E ad F, ita G ad H; erit rectangulum A in E ad rectangulum B in F, ut rectangulum

rectangulum C in G ad rectangulum D in H.

Est enim rectangulum A in E ad rectangulum B in E, ut A ad B, hoc est, ut C ad D, hoc est, ut rectangulum C in G ad rectangulum D in G; et est rectangulum B in E ad rectangulum B in F, ut E ad F, hoc est, ut G ad H, hoc est, ut rectangulum D in G ad rectangulum D in H: quare, ex aequo, erit rectangulum A in E ad rectangulum B in F, ut rectangulum C in G ad rectangulum D in H. Q. E. D.

PROP. II.

Fig. 5. Tab. 1.

Sit circulus ABC, et recta AB circulo occurrens in A, B; et sit C punctum in circumferentia circuli, et CA, CB jungantur, et sit punctum D in recta AB, sitque BE ad BC ut BD ad DA; per punctum D ducatur quaevis recta circulo occurrens in F, G; et CF, CG jungantur rectae AB occurrentes in H, K; erit rectangulum

lum HAK ad rectangulum HBK,
ut quadratum ex AC ad rectangu-
lum CBE.

Factum jam fit, et ducatur DL parallela
rectae AC rectis CB, CF, CG occurrens in
L, M, N; et per punctum L ducatur parallela
rectae AB rectis CF, CG occurrens in O, P.

Quoniam est rectangulum HAK ad rectan-
gulum HBK, ut quadratum ex AC ad re-
ctangulum CBE, erit, alternando, rectangu-
lum HAK ad quadratum ex AC, ut rectan-
gulum HBK ad rectangulum CBE. Quoni-
am vero est OL ad LM, ut HA ad AC, et
PL ad LN, ut KA ad AC; erit [per *Lem.*
praec.] rectangulum OLP ad rectangulum
MLN, ut rectangulum HAK ad quadratum
ex AC, hoc est, ut rectangulum HBK ad re-
ctangulum CBE; et, invertendo, erit rectan-
gulum MLN ad rectangulum OLP, ut re-
ctangulum CBE ad rectangulum HBK; est
autem rectangulum OLP ad quadratum ex
CL, ut rectangulum HBK ad quadratum ex
BC; quare erit rectangulum MLN ad qua-
dratum ex CL, ut rectangulum CBE ad qua-
dratum ex BC, hoc est, ut BE ad BC; et
quoniam est BE ad BC, ut BD ad DA, hoc
est,

est, ut BL ad LC, hoc est, ut rectangulum CLB ad quadratum ex CL; erit rectangulum MLN ad quadratum ex CL, ut rectangulum CLB ad quadratum ex CL: est igitur rectangulum MLN aequale rectangulo CLB. Quod quidem [per *Prop.* praec.] ita se habet.

Componetur autem sic. Quoniam est rectangulum MLN, aequale rectangulo CLB, erit rectangulum MLN ad quadratum ex CL, ut rectangulum CLB, ad quadratum ex CL, hoc est, ut BL ad LC; et quoniam est BL ad LC, ut BD ad DA, hoc est, ut BE ad BC, hoc est, ut rectangulum CBE ad quadratum ex BC, erit rectangulum MLN ad quadratum ex CL, ut rectangulum CBE ad quadratum ex BC: est autem quadratum ex CL ad rectangulum OLP, ut quadratum ex BC ad rectangulum HBK; quare erit rectangulum MLN ad rectangulum OLP, ut rectangulum CBE ad rectangulum HBK; et, invertendo, erit rectangulum OLP ad rectangulum MLN, ut rectangulum HBK ad rectangulum CBE. Quoniam vero est HA ad AC, ut OL ad LM, et KA ad AC, ut PL ad LN, erit [per *Lem.* praec.] rectangulum HAK ad quadratum ex AC, ut rectangulum OLP ad rectangulum MLN, hoc est, ut rectangulum

gulum HBK ad rectangulum CBE, et, alternando, erit rectangulum HAK ad rectangulum HBK, ut quadratum ex AC ad rectangulum CBE. Q. E. D.

PROP. III. *Tab. 1. Fig. 6.*

Sit circulus ABC et recta AB circulo occurrens in A, B; et sit C punctum in circumferentia circuli; et CA, CB jungantur, et sit punctum D in recta AB, sitque quaevis recta EF rectis CA, CB occurrens in E, F; et sit FG ad CF, ut DB ad DA; per punctum D ducatur quaevis recta circulo occurrens in H, K; et CH, CK jungantur rectae EF occurrentes in L, M; erit rectangulum LEM ad rectangulum LFM, ut quadratum ex EC ad rectangulum CFG.

Factum jam sit, et ducatur DN parallela rectae AC rectis CB, CH, CK occurrens in
N, O;

N, O, P; et per punctum N ducatur parallela rectae EF, rectis CO, CP occurrens in Q. R.

Quoniam est rectangulum LEM ad rectangulum LFM, ut quadratum ex EC ad rectangulum CFG, erit rectangulum LEM ad quadratum ex EC, ut rectangulum LFM ad rectangulum CFG. Quoniam vero est QN ad NO ut LE ad EC, et RN ad NP ut ME ad EC; erit [per *Lem.* ad *Prop.* 2.] rectangulum QNR ad rectangulum ONP, ut rectangulum LEM ad quadratum ex EC, hoc est, ut rectangulum LFM ad rectangulum CFG: et, invertendo, erit rectangulum ONP ad rectangulum QNR, ut rectangulum CFG ad rectangulum LFM: est autem rectangulum QNR ad quadratum ex CN, ut rectangulum LFM ad quadratum ex CF; quare erit rectangulum ONP ad quadratum ex CN, ut rectangulum CFG ad quadratum ex CF, hoc est, ut FG ad CF: et quoniam est FG ad CF ut DB ad DA, hoc est, ut BN ad NC, hoc est, ut rectangulum CNB ad quadratum ex CN; erit rectangulum ONP ad quadratum ex CN, ut rectangulum CNB ad quadratum ex CN: est igitur rectangulum ONP aequale rectangulo CNB. Quod quidem [per *Prop.* 1.] ita se habet.

U

Componetur

Componetur autem sic. Quoniam est rectangulum ONP aequale rectangulo CNB, erit rectangulum ONP ad quadratum ex CN, ut rectangulum CNB ad quadratum ex CN, hoc est, ut BN ad NC; et quoniam est BN ad NC ut BD ad DA, hoc est, ut GF ad FC, hoc est, ut rectangulum CFG ad quadratum ex CF; erit rectangulum ONP ad quadratum ex CN ut rectangulum CFG ad quadratum ex CF: est autem quadratum ex CN ad rectangulum QNR, ut quadratum ex CF ad rectangulum LFM; quare erit rectangulum ONP ad rectangulum QNR, ut rectangulum CFG ad rectangulum LFM: et, invertendo, erit rectangulum QNR ad rectangulum ONP, ut rectangulum LFM ad rectangulum CFG. Quoniam vero est LE ad EC ut QN ad NO, et ME ad EC ut RN ad NP; erit [per *Lem.* ad Prop. 2.] rectangulum LEM ad quadratum ex EC, ut rectangulum QNR ad rectangulum ONP, hoc est, ut rectangulum LFM ad rectangulum CFG: et, alternando, erit rectangulum LEM ad rectangulum LFM, ut quadratum ex EC ad rectangulum CFG. Q. E. D.

Corollarium.

Corrolarium. Sit circulus ABC (*Tab. 1. Fig. 6.*) et recta AB circulo occurrens in A, B; et sit C punctum in circumferentia circuli, et CA, CB jungantur; sitque recta EF rectis CA, CB occurrens in E, F; in AB producta sumatur punctum D, ut sit DB ad DA, ut quadratum ex CE ad quadratum ex CF; bifariam secetur EF in T, et per punctum D ducatur quaevis recta circulo occurrens in H, K, et CH, CK jungantur rectae EF occurrentes in L, M; erit LT aequalis ipsi TM.

Sit FG ad FC ut DB ad DA. Quoniam est rectangulum CFG ad quadratum ex CF, ut FG ad FC, hoc est, ut DB ad DA; et est DB ad DA ut quadratum ex CE ad quadratum ex CF: erit rectangulum CFG ad quadratum ex CF ut quadratum ex CE ad quadratum ex CF: est igitur quadratum ex CE aequale rectangulo CFG; est autem [per *Prop. praec.*] rectangulum LEM ad rectangulum LFM ut quadratum ex CE ad rectangulum CFG; quare erit rectangulum LEM aequale rectangulo LFM: erit igitur EL ad LF ut FM ad ME, et, componendo, erit EF ad FL ut EF ad EM; aequalis igitur est EM ipsi FL: et quoniam est ET aequalis ipsi TF, erit LT aequalis ipsi TM. Q. E. D.

PROP.

PROP. IV. *Tab. 1. Fig. 7.*

Sit recta AB circulo occurrens in A, B; et sint rectae AC, BC circulum contingentes in A, B; et per quodvis punctum D in AB ducatur DE parallela rectae AC, rectae BC occurrens in E. Si per punctum D ducatur quaevis recta circulo occurrens in F, G; et CF, CG jungantur, rectae DE occurrentes in H, K; erit rectangulum HEK aequale quadrato ex DE.

Factum jam sit. Jungantur CD; et occurrat recta DF rectis AC, BC in L, M; per M ducatur recta rectis AC, DE parallela; occurratque rectis CH, CE, CD, et AD, in N, O, P et Q punctis.

Quoniam est rectangulum HEK aequale quadrato ex DE, et est rectangulum NMO ad quadratum ex MP ut rectangulum HEK ad quadratum ex DE; erit rectangulum NMO aequale quadrato ex MP: et est rectangulum
FMG

FMG aequale quadrato ex MB; quare erit rectangulum FMG ad rectangulum NMO, ut quadratum ex MB ad quadratum ex MP: et quoniam est FL ad LC ut FM ad MN, et GL ad LC ut GM ad MO; erit [per *Lem.* ad prop. 2.] rectangulum FLG, hoc est, quadratum ex AL, ad quadratum ex LC, ut rectangulum FMG ad rectangulum NMO, hoc est, ut quadratum ex MB ad quadratum ex MP: est autem quadratum ex MQ ad quadratum ex MP, ut quadratum ex AL ad quadratum ex LC, hoc est, ut quadratum ex MB ad quadratum ex MP; quare erit MQ aequalis rectae MB: est autem AC ad CB, ut MQ ad MB; aequalis igitur est recta AC rectae CB. Quod quidem verum est, quoniam rectae AC, BC circulum contingunt in A, B punctis.

Componetur autem sic. Quoniam rectae AC, BC circulum contingunt in A, B punctis, erit recta AC aequalis rectae BC: et est MQ ad MB ut AC ad CB; quare erit MQ aequalis rectae MB; est igitur quadratum ex MQ aequale quadrato ex MB, hoc est rectangulo FMG; quare erit rectangulum FMG ad rectangulum NMO ut quadratum ex MQ ad rectangulum NMO: et quoniam est FL ad LC ut FM ad MN, et GL ad LC ut GM ad MO;

MO; erit rectangulum FLG, hoc est, quadratum ex AL ad quadratum ex LC ut rectangulum FMG ad rectangulum NMO, hoc est, ut quadratum ex MQ ad rectangulum NMO: est autem quadratum ex MQ ad quadratum ex MP ut quadratum ex AL ad quadratum ex LC, hoc est, ut quadratum ex MQ ad rectangulum NMO; aequale igitur est rectangulum NMO quadrato ex MP: et quoniam est rectangulum HEK ad quadratum ex DE ut rectangulum NMO ad quadratum ex MP, erit rectangulum HEK aequale quadrato ex DE. Q. E. D.

P R O P. V. *Tab. 1. Fig. 8.*

Sit recta AB circulo occurrens in A, B, et sint rectae AC, BC circulum contingentes in A, B; per punctum quodvis D in AB ducatur recta circulo occurrens in F, G; et CF, CG jungantur, rectae AB occurrentes in H, K; erit rectangulum HAK ad rectangulum HBK, ut quadratum ex AD ad quadratum ex DB.

Factum

Factum jam sit. Et ducatur DL parallela rectae AC, rectis CB, CF, CG occurrens in L, M, N; et ducatur LO parallela rectae AB, rectis CF, CG occurrens in O, P.

Quoniam est rectangulum HAK ad rectangulum HBK, ut quadratum ex AD ad quadratum ex DB, hoc est, ut quadratum ex CL ad quadratum ex LB; et est rectangulum HBK ad rectangulum OLP, ut quadratum ex CB ad quadratum ex CL; erit rectangulum HAK ad rectangulum OLP, ut quadratum ex CB vel AC ad quadratum ex BL: et, alternando, erit rectangulum HAK ad quadratum ex AC, ut rectangulum OLP ad quadratum ex BL. Rursus, quoniam est OL ad LM ut HA ad AC, et PL ad LN ut KA ad AC; erit [*Lem. ad prop. 2.*] rectangulum OLP ad rectangulum MLN, ut rectangulum HAK ad quadratum ex AC, hoc est, ut rectangulum OLP ad quadratum ex BL. Est igitur rectangulum MLN aequale quadrato ex BL, hoc est, quadrato ex DL. Quod quidem [*Prop. praec.*] verum est.

Componetur autem sic. Quoniam est rectangulum MLN aequale [*Prop. praec.*] quadrato ex DL, hoc est, quadrato ex BL, erit rectangulum OLP ad rectangulum MLN

ut

ut rectangulum OLP ad quadratum ex BL : quoniam vero est HA ad AC ut OL ad LM, et KA ad AC ut PL ad LN; erit rectangulum [per *Lem.* ad prop. 2.] HAK ad quadratum ex AC, ut rectangulum OLP ad rectangulum MLN, hoc est, ut rectangulum OLP ad quadratum ex BL : et alternando, erit rectangulum HAK ad rectangulum OLP, ut quadratum ex AC vel BC ad quadratum ex BL : est autem rectangulum OLP ad rectangulum HBK, ut quadratum ex CL ad quadratum ex CB; quare erit rectangulum HAK ad rectangulum HBK, ut quadratum ex CL ad quadratum ex BL, hoc est, ut quadratum ex AD ad quadratum ex DB. Q. E. D.

PROP. VI. *Tab. 1. Fig. 9.*

Sint duae rectae AC, BC, circulum contingentes in A, B, et sit D punctum in recta AB; sit recta EF, rectis AC, BC occurrens in E, F, et juncta CD in G: si per punctum D ducatur quaevis recta circulo occurrens in H, K, et CH, CK jungantur, rectae EF occurrentes in L, M; erit

erit rectangulum LEM ad rectangulum LFM, ut quadratum ex GE ad quadratum ex GF.

Factum jam fit. Per punctum D ducatur recta parallela rectae AC, rectis CB, CH CK occurrens in N, O, P; per punctum N ducatur recta parallela rectae EF, rectis CH, CK occurrens in Q, R; et per punctum F ducatur FS parallela rectae AC, rectae CD occurrens in S.

Quoniam est rectangulum LEM ad rectangulum LFM, ut quadratum ex EG ad quadratum ex FG, hoc est, ut quadratum ex EC ad quadratum ex FS; erit, alternando, rectangulum LEM ad quadratum ex EC, ut rectangulum LFM ad quadratum ex FS: quoniam est rectangulum QNR ad quadratum ex CN, ut rectangulum LFM ad quadratum ex CF, et est quadratum ex CN ad quadratum ex ND, ut quadratum ex CF, ad quadratum ex FS; erit rectangulum QNR ad quadratum ex ND, ut rectangulum LFM ad quadratum ex FS, hoc est, ut rectangulum LEM ad quadratum ex EC. Quoniam vero est LE ad EC ut QN ad NO, et ME

X

ad

ad EC ut RN ad NP, erit [per *Lem* ad prop. 2.] rectangulum LEM ad quadratum ex EC, ut rectangulum QNR ad rectangulum ONP; quare erit rectangulum QNR ad rectangulum ONP, ut rectangulum QNR ad quadratum ex ND: est igitur rectangulum ONP aequale quadrato ex ND. Quod quidem [per *Prop.* 4.] verum est.

Componetur autem sic. Quoniam est rectangulum ONP aequale quadrato ex ND, erit rectangulum QNR ad rectangulum ONP ut rectangulum QNR ad quadratum ex ND; et quoniam est LE ad EC ut QN ad NO, et ME ad EC ut RN ad NP, erit rectangulum LEM ad quadratum ex EC, ut rectangulum QNR ad rectangulum ONP, hoc est, ut rectangulum QNR ad quadratum ex ND. Quoniam vero est rectangulum QNR ad quadratum ex CN, ut rectangulum LFM ad quadratum ex CF, et est quadratum ex CN ad quadratum ex ND, ut quadratum ex CF ad quadratum ex FS; erit rectangulum QNR ad quadratum ex ND, ut rectangulum LFM ad quadratum ex FS; quare erit rectangulum LEM ad quadratum ex EC, ut rectangulum LFM ad quadratum ex FS: et, alternando, erit rectangulum LEM ad rectangulum LFM,

ut

ut quadratum ex EC ad quadratum ex FS,
hoc est, ut quadratum ex EG ad quadratum
ex FG. Q. E. D.

P R O P. VII. *Tab. 2. Fig. 10.*

Sit recta AB circulo occurrens in A,
B, et in ipsa sumatur punctum C,
et ducatur CD circulo occurrens in
D, E, et AD, BE jungantur sibi
mutuo occurrentes in F; ducatur
CG parallela rectae AF, rectae BF
occurrens in G: si per punctum C
ducatur quaevis recta circulo occur-
rens in H, K, et FH, FK jungantur,
rectae CG occurrentes in L, M; erit
rectangulum LGM aequale quadra-
to ex CG.

Factum jam sit. Jungantur CF, et oc-
currat recta CH rectis AF, BF in N, O; per
punctum O ducatur recta parallela rectae CG,
vel AF, rectis FL, FM, FC occurrens in P,
Q, R, et rectis AB, DE in S, T.

Quoniam

Quoniam est rectangulum LGM aequale quadrato ex CG, et est rectangulum POQ ad quadratum ex OR, ut rectangulum LGM ad quadratum ex CG; erit rectangulum POQ aequale quadrato ex OR: et est rectangulum HOK aequale rectangulo EOB; quare erit rectangulum HOK ad rectangulum POQ, ut rectangulum EOB ad quadratum ex OR: et, quoniam est HN ad NF ut HO ad OP, et KN ad NF ut KO ad OQ; erit [per *Lem.* ad prop. 2) rectangulum HNK ad quadratum ex FN, ut rectangulum HOK ad rectangulum POQ, hoc est, ut rectangulum EOB ad quadratum ex OR: est autem quadratum ex FN ad rectangulum AND, ut quadratum ex OR ad rectangulum SOT; quare erit rectangulum HNK ad rectangulum AND, ut rectangulum EOB ad rectangulum SOT: et quoniam est rectangulum HNK aequale rectangulo AND, erit rectangulum EOB aequale rectangulo SOT; quare erit OB ad OS ut OT ad OE, hoc est, erit BG ad GC ut GC ad GE: est igitur angulus GEC aequalis angulo GCB, hoc est, angulo BAD; quod quidem verum est, quoniam puncta A, B, E, D sunt in circulo.

Componetur

Componetur autem sic. Quoniam puncta A, B, E, D sunt in circulo, erit angulus GEC aequalis angulo BAD, hoc est, angulo GCB; quare erit BG ad GC ut GC ad GE, hoc est, erit OB ad OS ut OT ad OE: est igitur rectangulum SOT aequale rectangulo EOB, hoc est, rectangulo HOK; quare erit rectangulum HOK ad quadratum ex OR, ut rectangulum SOT ad quadratum ex OR, hoc est, ut rectangulum AND, vel HNK, ad quadratum ex FN. Quoniam vero est HN ad NF ut HO ad OP, et KN ad NF ut KO ad OQ, erit rectangulum HNK ad quadratum ex FN, ut rectangulum HOK ad rectangulum POQ; quare erit rectangulum HOK ad rectangulum POQ ut rectangulum HOK ad quadratum ex OR; est igitur rectangulum POQ aequale quadrato ex OR: et quoniam est rectangulum LGM ad quadratum ex GC, ut rectangulum POQ ad quadratum ex OR, erit rectangulum LGM aequale quadrato ex GC. Q. E. D.

PROP. VIII. *Tab. 2. Fig. 11.*

Sit recta AB circulo occurrens in A, B, et in AB sumatur punctum C, ducatur

ducatur CD circulo occurrens in D, E, et jungantur AD, BE, sibi mutuo occurrentes in F; per punctum C ducatur quaevis recta circulo occurrens in G, H, et FG, FH jungantur, rectae AB occurrentes in K, L; erit rectangulum KAL ad rectangulum KBL, ut quadratum ex AC ad quadratum ex BC.

Factum jam sit. Ducatur CM parrallela rectae AF, rectis FB, FK, FL occurrens in M, N, O; et per punctum M ducatur parrallela rectae AB, rectis FK, FL occurrens in P. Q.

Quoniam est rectangulum KAL ad rectangulum KBL, ut quadratum ex AC ad quadratum ex BC, hoc est, ut quadratum ex FM ad quadratum ex BM; et est rectangulum KBL ad rectangulum PMQ, ut quadratum ex FB ad quadratum ex FM; erit rectangulum KAL ad rectangulum PMQ, ut quadratum ex FB ad quadratum ex BM, hoc est, ut quadratum ex AF ad quadratum ex CM; et, alternando, erit rectangulum KAL ad

ad quadratum ex AF, ut rectangulum PMQ ad quadratum ex MC. Quoniam vero est PM ad MN ut KA ad AF, et QM ad MO ut LA ad AF; erit [per *Lem.* ad prop. 2.] rectangulum PMQ ad rectangulum NMO, ut rectangulum KAL ad quadratum ex AF, hoc est, ut rectangulum PMQ ad quadratum ex CM: est igitur rectangulum NMO aequale quadrato ex CM. Quod quidem [per *Prop.* praec.] verum est.

Componetur autem sic. Quoniam est rectangulum NMO aequale quadrato ex MC, erit rectangulum PMQ ad rectangulum NMO ut rectangulum PMQ ad quadratum ex MC: quoniam vero est KA ad AF ut PM ad MN, et LA ad AF ut QM ad MO; erit rectangulum KAL ad quadratum ex AF, ut rectangulum PMQ ad rectangulum NMO, hoc est, ut rectangulum PMQ ad quadratum ex MC: et, alternando, erit rectangulum KAL ad rectangulum PMQ, ut quadratum ex AF ad quadratum ex MC, hoc est, ut quadratum ex FB ad quadratum ex BM: et est rectangulum PMQ ad rectangulum KBL, ut quadratum ex FM ad quadratum ex FB; quare erit rectangulum KAL ad rectangulum KBL ut est

quadratum ex FM ad quadratum ex BM, hoc est ut quadratum ex AC ad quadratum ex BC, Q. E. D.

PROP. IX. *Tab. 2. Fig. 12.*

Sit recta AB circulo occurrens in A, B, et in AB sumatur punctum C; per C ducatur recta circulo occurrens in D, E; et AD, BE jungantur, sibi mutuo occurrentes in puncto F; et sit recta GH rectis AF, BF occurrens in G, H, et juncta FC in K: si per punctum C ducatur quaevis recta circulo occurrens in L, M, et FL, FM jungantur, rectae GH occurrentes in N, O; erit rectangulum OGN ad rectangulum OHN, ut quadratum ex GK ad quadratum ex HK.

Factum jam sit. Ducatur per punctum C parallela rectae AF rectis FB, FL, FM occurrens in P, Q, R; et per punctum P ducatur

catur parallela rectae GH rectis FL, FM occurrens in S, T, et ducatur HV parallela rectae PC rectae FC occurrens in V.

Quoniam est rectangulum OGN ad rectangulum OHN, ut quadratum ex GK, ad quadratum ex HK, hoc est, ut quadratum ex FG ad quadratum ex HV; erit, alternando, rectangulum OGN ad quadratum ex FG, ut rectangulum OHN ad quadratum ex HV: quoniam est rectangulum SPT ad quadratum ex PF, ut rectangulum OHN ad quadratum ex HF, et est quadratum ex PF ad quadratum ex PC, ut quadratum ex HF ad quadratum ex HV; erit rectangulum SPT ad quadratum ex PC, ut rectangulum OHN ad quadratum ex HV, hoc est, ut rectangulum OGN ad quadratum ex FG. Quoniam vero est NG ad GF ut SP ad PQ, et OG ad GF ut TP ad PR, erit [per *Lem.* ad prop. 2.] rectangulum OGN ad quadratum ex FG, ut rectangulum SPT ad rectangulum QPR; quare erit rectangulum SPT ad rectangulum QPR, ut rectangulum SPT ad quadratum ex PC. Est igitur rectangulum QPR aequale quadrato ex PC. Quod quidem [per *Prop.* 7.] verum est.

Componetur autem sic. Quoniam est rectangulum QPR aequale quadrato ex PC, erit

Y rectangulum

rectangulum SPT ad rectangulum QPR, ut
 rectangulum SPT ad quadratum ex PC: et
 quoniam est NG ad FG ut SP ad PQ, et OG
 ad GF ut TP ad PR, erit rectangulum OGN
 ad quadratum ex FG, ut rectangulum SPT
 ad rectangulum QPR, hoc est, ut rectangu-
 lum SPT ad quadratum ex PC. Quoniam
 vero est rectangulum SPT ad quadratum ex
 PF, ut rectangulum OHN ad quadratum ex
 HF, et est quadratum ex FP ad quadratum
 ex PC, ut quadratum ex HF ad quadratum
 ex HV; erit rectangulum SPT ad quadratum
 ex PC, ut rectangulum OHN ad quadratum
 ex HV: quare erit rectangulum OGN ad
 quadratum ex FG, ut rectangulum OHN ad
 quadratum ex HV: et, alternando, erit re-
 ctangulum OGN ad rectangulum OHN, ut
 quadratum ex FG ad quadratum ex HV, hoc
 est, ut quadratum ex GK ad quadratum ex
 HK. Q. E. D.

SCHOLIUM.

*Propositiones ante dictae in sectionibus quoque
 conicis obtinent. Idque reputanti sequentia ob-
 servata sunt theoremata, a quibus alia etiam
 multa deduci possunt.*

THEOR.

THEOR. 1. *Sint* [Tab. 2. Fig. 13.] *duae rectae* AB, CD, *sectionem conicam contingentes in* A, C; *et* A, C *jungantur*; *sit quaevis recta, rectis* AB, CD, AC *occurrens in* B, D, E, *et sectioni in* F, G *punctis*; *erit rectangulum* BFD *ad rectangulum* BGD, *ut quadratum ex* EF *ad quadratum ex* EG.

THEOR. 2. *Sit* [Tab. 2. Fig. 14.] *recta* AB *sectionem conicam contingens in* A, *et recta* CD *sectioni occurrens in* C, D *punctis*; *et jungantur* AC, AD; *occurratque quaevis recta rectis* AB, CD, AC, AD *in* E, F, G, H, *et sectioni in* K, L *punctis*; *erit rectangulum* EKF *ad rectangulum* ELF, *ut rectangulum* GKH *ad rectangulum* GLH.

THEOR. 3. *In* [Tab. 2. Fig. 15.] *sectione conica inscribantur duae rectae* AB, CD; *et jungantur* AC, BD; *occurratque quaevis recta rectis* AB, CD, AC, BD, *in* E, F, G, H, *et sectioni in* K, L *punctis*; *erit rectangulum* EKF *ad rectangulum* ELF, *ut rectangulum* GKH *ad rectangulum* GLH.

Ex hisce patet sequens theorema.

Si in sectione conica inscribatur quaevis figura quadrilatera, cujus tria latera per data puncta in recta transeunt; reliquum latus, vel
ad

ad datum punctum verget, vel parallelum erit positione datae.

Ut finem faciam; theorema unum tantum subijciam, observatione quidem haud indignum; et quod etiam in sectionibus conicis, paululum mutatum, obtinet; unde alia, à nullis, quod sciam, scriptoribus geometricis observata, deriventur.

Sit AB diameter circuli [Tab. 2. Fig. 16. 17.] positione dati, occurratque recta CD ipsi normalis in dato puncto C; non sit autem C centrum circuli, et datum sit punctum D in recta CD; sitque quadratum ex CD majus rectangulo ACB, si sit punctum C intra circulum; si vero extra, sit quadratum ex CD minus rectangulo ACB; producat DC ad E, ut sit CE aequalis ipsi CD; dabuntur duo puncta F, G extra rectam DE, ut, si ab ipsis inflectantur rectae FH, GH ad punctum quodvis H in circulo, rectae DE occurrentes in K, L, erit summa quadratorum ex DK, EK ad rectangulum DLE in ratione data.

ART.

Fig. 1.

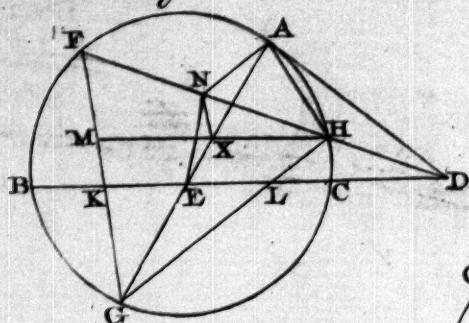


Fig. 4.

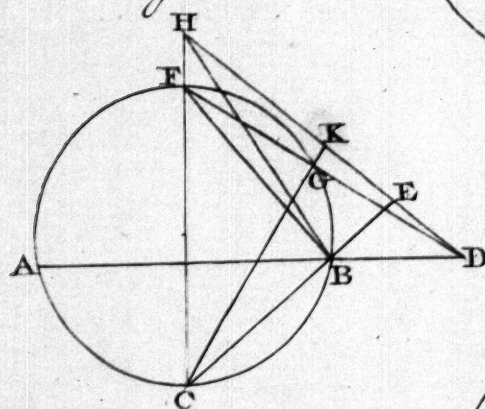
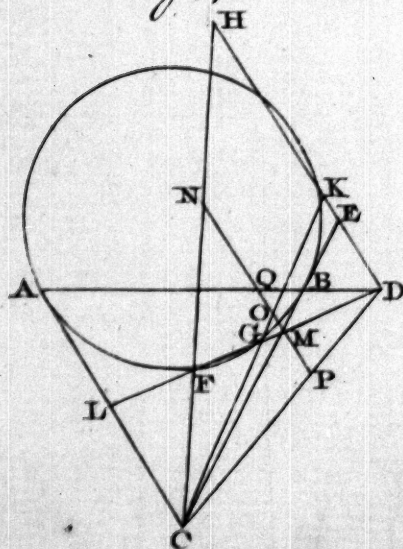


Fig. 7.



TAB. I.

Fig. 3. 142

Fig. 2.

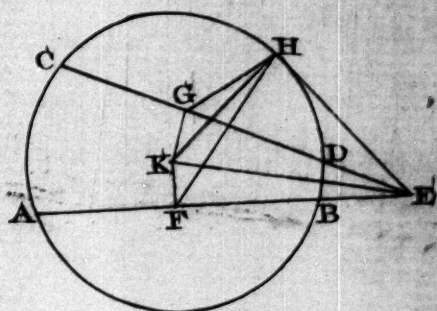
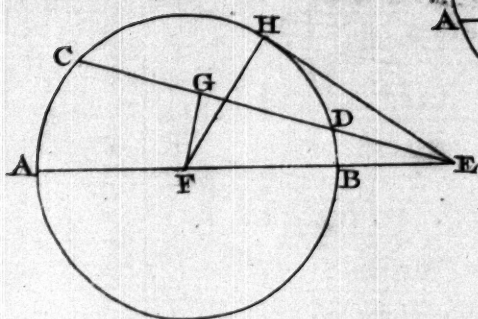


Fig. 5.

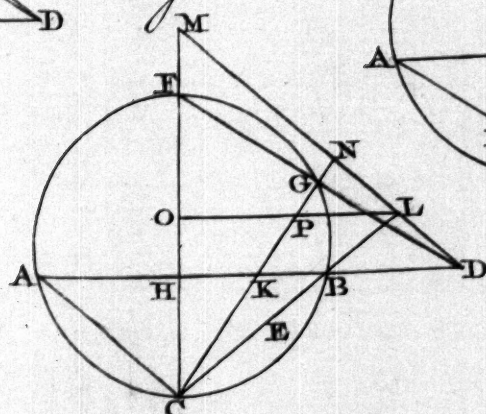


Fig. 6.

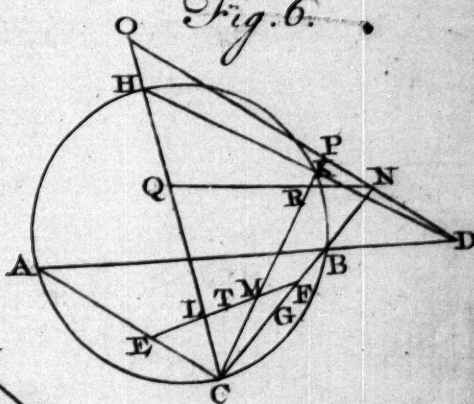


Fig. 8.

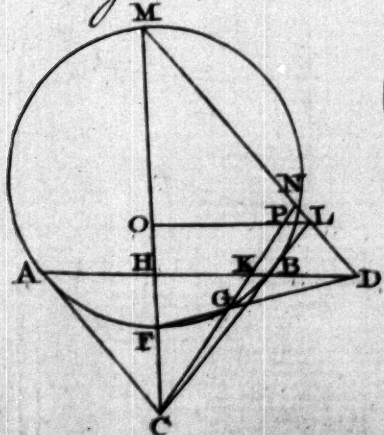
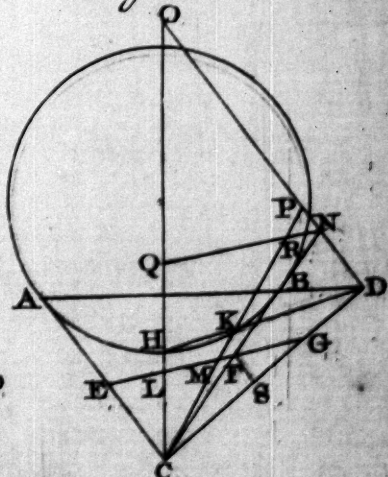


Fig. 9.



ART. IV.

Of the Cause of the Variation of the Obliquity of the Ecliptic, by COLIN McLAURIN, late Fellow of the Royal Society, and Professor of Mathematics in the University of Edinburgh.

THE French astronomers in Peru, and Dr. Bevis at London, and others elsewhere, have endeavoured to determine the obliquity of the ecliptic with greater accuracy than former astronomers had attained to. The result of their observations makes it to be now about $23^{\circ} 29' 28'' \frac{1}{3}$. It had been for some time reckoned $23^{\circ} 29' 29''$; and before that, $23^{\circ} 29' 30''$; and, by some antient astronomers, 19 or 20 minutes greater. Mess. *Cassini* and *Bradley* had found it to vary a little; and this has given occasion to a notion, that it is decreasing continually; that there was a time when the axis of the earth lay in the plane of the earth's orbit; that it has been gradually rising, till now it contains an angle with this plane of $66^{\circ} 30' 31'' \frac{2}{3}$; and that, in

a long period of time, it will become perpendicular to this plane ; when we shall have everywhere a continual spring throughout the year. The Chevalier *de Louville* and M. *Godin* pretend even to compute the period of this motion ; the former having gone to *Marseilles* to measure the variation of this obliquity from the time when *Pitheas* measured it at the same place 2000 years ago ; he found it diminished by 20', and thence estimates the period of this motion to be above 2 millions of years. M. *Godin* has since found, that, from the year 1655 to 1734, it decreased 55", by comparing an observation of old M. *Cassini*'s with his own ; and thence deduces the period to be 1944000 years. And hence some authors have endeavoured to explain several old *Egyptian* and *Babylonish* traditions, concerning the sun's having risen twice in the west ; and have taken occasion to indulge themselves in several speculations.

I HAVE formerly seen a piece that was printed many years ago, but never, I think, published ; containing a system and a kind of liturgy for the Pantheists or Spinofists ; wherein the author warmly espoused this notion, concerning the variation of the obliquity

ty

ty of the ecliptic, and endeavoured to improve it, for supporting his favourite principle of equality ; imagining, that, in the compass of this period, the state and phænomena of all places on the earth would be upon the whole equal ; an opinion that seems to suit the patrons of a blind and absolute necessity : but, whatever be thought of this continual and regular diminution of the angle in which the ecliptic and equator intersect each other, it is certain that such an equality would not be the consequence of it. Places of the earth would still have their peculiarities : the people at the equator would have their days and nights constantly equal, how great or small soever the obliquity of the ecliptic were ; for this is a necessary consequence of their describing a great circle of the earth by the diurnal motion, that is always bisected by the boundary of light and darkness, which is likewise a great circle in consequence of a necessary truth. The poles would have their six months day and six months night, as now ; with this further singularity, that, when the axis lay in the plane of the ecliptic, the heat must have been far more intolerable at the poles than is now known in any part of the earth,

earth, or could be known in any other part of it, during this long period : for the sun must have then continued ten days together within 5° degrees of the zenith of the poles, without any intermission of night, or abatement from a lesser elevation, at which altitude he never continues above $40'$ in the equinoctial days at the equator ; a peculiarity as singular as it would have been fatal to the polar regions. The present obliquity of the ecliptic gives evidently great relief to the equatoreal regions ; as by it the sun is carried to both sides of the zenith during the year, and is not permitted to dart constantly perpendicular rays upon them. It is to it likewise the more northern countries owe their summer and harvest ; which we are told is over with them in two months, or very little more, while the sun's almost continued and incessant action ripens fruits which the heat of our sun, much less theirs, on the 10th of *March*, the equinoctial day, could never be able to accomplish. Thus it serves for diminishing their heat at the equator where it was useful to lessen it, and for increasing it towards the polar regions where it was proper to strengthen it. But a regular
and

and continued variation of the obliquity of the ecliptic would ruin some parts of the world, and disturb an order well established in nature, without serving the purpose of the Pantheists.

I WOULD not take up your time in making remarks on what is brought to support this doctrine, from old fables of *Egyptian* or *Babylonish* priests delivered to *Herodotus*, that seem to have flown from their vanity. What is shewn by *Dr. Shaw*, in his travels, concerning the gradual rising of the surface of the land of *Egypt*, from the annual sediments of the *Nile*, seems to afford a much better argument against their boasted antiquity in *Egypt*, than any thing can be drawn from astronomical observations, to support a vanity so universal, that we need not go very far from home to find absurd enough instances of it. As for what is said of *Pitheas* and other antients, their wrong notions concerning the refractions and the imperfection of their instruments, render any conclusions on so nice a subject, drawn from their observations, dubious. We ought to shew more regard to the modern observations brought for this doctrine, than to represent

Z

them

them in a level with the fables vented by the priests of *Egypt*. According to several modern observations, the obliquity of the ecliptic varies : but we are to remember, that even the noble *Tycho Brache* committed an error of 18', in drawing his meridian in his island of *Teraniburgum* ; and, from his erroneous opinions concerning the refraction of the atmosphere, made the mean distance of the moon less by 4 semidiameters of the earth than it really is. It is true, Mr. *Cassini's* observation in 1655, compared with late ones, makes it vary at the rate of 55" in 79 years. Let us therefore see, if a variation, such as that arises from Mess. *Cassini's* and *Godin's* observations, be not a consequence of principles already established in the solar system ; and offer some hints of a method by which it may be ascertained whether this be the just solution of this question, and how, from a proper series of observations, made with great exactness, an improvement and correction in geography and navigation may be deduced from this inquiry, by bringing the theory of the earth's motion in her orbit, and particularly what relates to the declination of the sun to greater perfection. This will be the

more

more useful, that the usual methods of taking the latitude of places by the sun's meridian altitudes, supposes the tables of his declination to be perfectly just ; whereas there is ground to doubt of their preciseness.

MATHEMATICAL instruments in general, and such particularly as serve for astronomical purposes, have been, of late, brought to an exactness never known in former times. Hence some minute motions, or inequalities of motions, in the heavens, may be discovered, that have hitherto escaped the most diligent observers. They had discovered indeed some motions that are very minute or slow, such as the procession of the equinoxes ; but this was owing to the continual increase of the effects of those very minute motions, which, tho' insensible in one revolution, become very manifest in many revolutions. Motions of this kind are easily found, and are accurately determined, by comparing distant observations. But there are other minute motions which are hard to be discovered : these which do not grow up to be sensible in a number of revolutions ; their effect in one part destroying their effect in another part of a revolution. Of this kind is the motion

tion lately discovered by Mr. *Bradley*, which, on this account, lay so long concealed from astronomers. Of the same kind is the motion of the fixed stars, that ought to arise from the parallax of the earth's orbit. And of this kind I suspect the variation of the obliquity of the ecliptic to be.

To proceed therefore to what I take to be the cause of this motion, Sir *Isaac Newton* has found, that the sun and planets gravitate all towards each other mutually ; that it is neither the sun (according to the *Copernican* system), nor the earth (according to *Ptolemy*), that is the center of the system, or fixed point ; but the center of gravity of the whole system. That the sun therefore moves about this center : and that when *Jupiter* and *Saturn*, the two biggest planets, are in the same right line, on the same side of the sun ; the center of the sun will be, almost a diameter of the sun, distant from this fixed point. Hence, tho' we suppose the earth to move always in the same plane, the sun will appear to have different declinations at the time of the solstice : and, as the obliquity of the ecliptic is determined always from the declination of the sun at the solstice, it will
therefore

therefore appear to vary, but not in a manner that will accrefce and produce any fenfible change in our feafons ; but it will increafe and decreafe a little within fmall limits, and its variation will principally depend on the pofition of *Jupiter* and *Saturn* to the fun and earth, and will nearly return to the fame magnitude when thefe return to the fame pofition, with refpect to one another, and to the folftice. *Jupiter* has moft effect in producing this variation ; and, if it be found to have a connexion or dependence on his pofition to the fun and earth, it will be an indication that this is the true caufe of the phænomenon.

WAS the orbit of the earth perpendicular to the orbit of *Jupiter*, this variation would be much more confiderable than it is, and might amount to above half a degree, or a diameter of the fun. Suppofe [*Tab. 2. Fig. 18.*] BET to represent the orbit of the earth in fuch a cafe, C the common center of gravity of the fun and *Jupiter*, which we may confider as the center of the earth's orbit, becaufe of the minutenefs of the earth compared with thefe, T the folftitial point, PT ρ the axis of the earth, IS the right line joining

ing the centers of the sun and *Jupiter*, which we suppose perpendicular to the orbit of the earth, while the earth is in the solstice T. Then, if *Jupiter* be at I, and the sun at S; the angle PTS will be the complement of the sun's greatest declination, or of the angle contained by the ecliptic and equator: but, if *Jupiter* be at *i*, and the sun at *f*; the angle PT*f* will be the complement of this declination, and these would differ by the angle ST*f*, which, (because CS is nearly equal to a semidiameter of the sun) might amount to 32'.

BUT, because the angle contained by the orbits of *Jupiter* and the earth is small, the right line CS is seen very obliquely from the earth, and the variation of the obliquity thence arising is small. The angle CTS is nearly to the inclination of *Jupiter*'s plane to the ecliptic, as the semidiameter of the sun to the distance of the earth from the sun: or the angle CTS is about $\frac{1}{17}$, of that inclination; therefore ST*f*, which is equal to 2CTS, may be somewhat more than a minute. And this may be sufficient to account for any variation that is yet discovered by astronomers. But, since it may exceed one minute, it appears that

TAB. II.

Fig. 11.

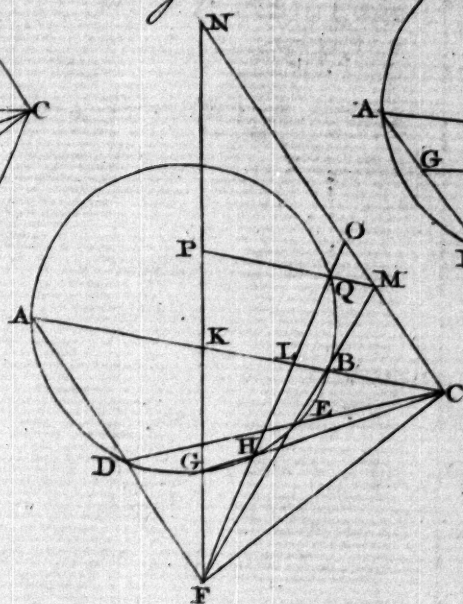


Fig. 12.

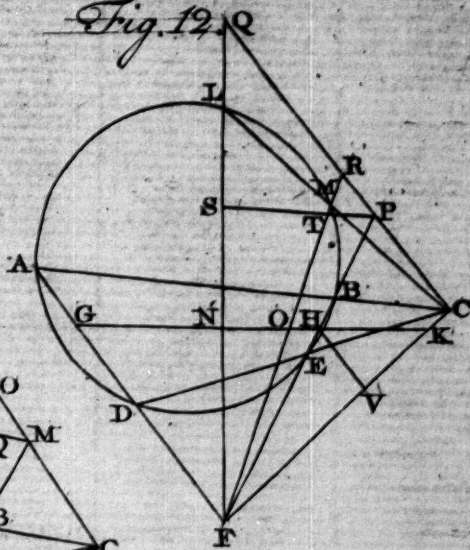


Fig. 14.

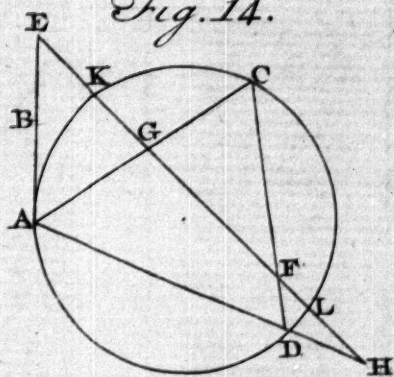


Fig. 15.

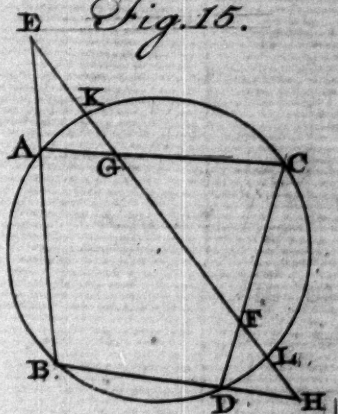


Fig. 17.

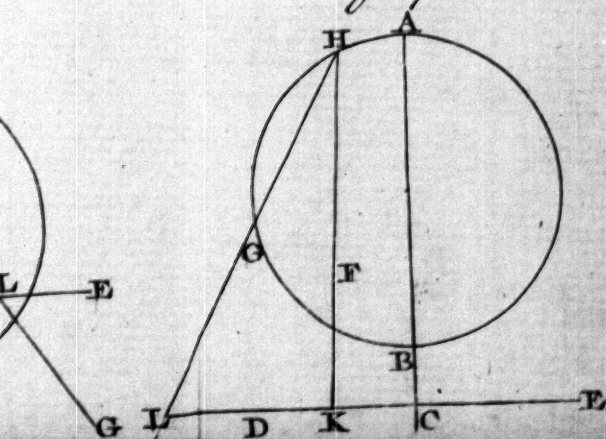
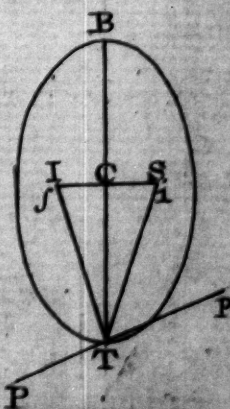


Fig. 18.



that it is of moment to have this theory prosecuted farther and ascertained : for an error of a minute, in a matter of such moment as the declination of the sun, which is an element in most questions in astronomy, geography, and navigation, is not to be despised ; especially now when so great exactness in these matters is desired, and, I believe, may be obtained. Such a variation, however, can have no sensible effect on our seasons ; and there seems to be no ground to apprehend, that our year will degenerate into a perpetual spring in any period of time ; which, after all, would be far from being an advantage to us, or to the inhabitants of the earth in general.

ART.

ART. V.

*Concerning the sudden and surprising Changes
observed in the Surface of Jupiter's Body ;
by the same.*

THERE have been more sudden and surprising changes observed on the surface of *Jupiter's* body, than on any other planet in the system. *Cassini* has observed several belts rise on his body in one hour's time ; and spots, in very different forms, are often seen upon him. I could wish that the position of his satellites were well considered when those changes happen : for, since *Jupiter* has four moons, some of which are much nearer to him than our moon is to the earth ; when they are all in conjunction or opposition to one another, very great tides must be produced in the ocean there, if there be any ; and there is ground to suspect that these sudden and great changes on his surface are owing to tides analogous to those we call

call spring tides. Further, the velocity with which *Jupiter* revolves on his axis, and the greatness of his body, must contribute to the greatness of these tides, and of the inundations produced by them.

IN *Jupiter's* system, the theory of gravity discovers itself in some particulars more manifestly than in any other part of the general system. *Jupiter* is the only body that is, to sense, spheroidical; the axis being less than the diameter of the equator by $\frac{1}{12}$. Possibly we may, by farther observations, discover the effects of the tides likewise; or rather may learn, that effects already visible and known proceed from them.

THE action of the satellites upon one another produces some irregularities in their motions; and from this it is supposed that their eclipses do not answer accurately to the times computed from the tables. But there is another source, I believe, of their irregularities. *Jupiter's* figure differs considerably from a sphere; and the gravitation towards him is not inversely as the square of the distance from his center. Now this variation from the regular course of gravity may produce some inequalities in their motions. And,

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in order to judge of these, I have computed the law of gravity towards such a spheroid as *Jupiter*; and will subjoin some theorems on this subject.

IT seems somewhat extraordinary, that in 130 years, since *Jupiter*'s satellites were discovered by *Galileo*, no eclipse of any of these satellites by one another has been observed. Such an eclipse could hold but for a small time: but, as it is not difficult to compute nearly the time when they ought to happen, it would be worth while to look for them; since this might contribute to render their theory more perfect; which is of so much use in ascertaining the longitudes of places. If it be found to be a phenomenon that passes over quickly, because of their swift motions; it may be the more useful, providing it can be seen.

No phenomenon seems more extraordinary, relating to *Jupiter*, than that, according to *Cassini*, some of his spots revolve in less time than others; the difference amounting to 4'. This is a phenomenon, of that kind, of which it is perhaps best not to attempt any explication, till the same be confirmed by more observations.

IT

It is worthy of our notice, that, from several phenomena, astronomers conclude the satellites to revolve on their axis, in the same time that they revolve around their primaries; by which means the exceeding great tides that would be produced in them are avoided. Those arising from their various distances from the primaries, in their *apsides*, may be sufficient for agitating their waters. The tides that would be produced in our moon, from this circumstance alone, ought to be considerably greater than the tides produced in our ocean; and possibly, by a careful attention to the limits of those black parts of the moon which formerly were considered as seas, but, because of cavities discovered in them, with better telescopes, are suspected by many to be only large planes, some decision of this question might be obtained. Water is of too great importance, in natural operations, to suppose hastily any planet to be deprived of it; tho' we must also allow that the variety of nature is not to be limited by our conceptions.

THEOR.

THEOR. I.

THE gravity towards an oblate spheroid in the axis produced, decreases in a less proportion than that in which the square of the distance from the center increases : but the gravity in the plane of the equator decreases in a higher proportion than that in which the square of the distance increases.

THEOR. II.

LET b denote the semidiameter of the equator, c the distance of the focus of the generating ellipse from the center, d the distance of any body from the center of the spheroid in the plane of the equator; and the gravity at the distance d , will be to the gravity at the distance b , as $\frac{b^2}{d^2} \times 1 + \frac{3c^2}{10d^2} + \frac{9c^4}{56d^4}$
 $\mathcal{G}d$ to $1 + \frac{3c^2}{10b^2} + \frac{9c^4}{56b^4}$ $\mathcal{G}c$. * * *

ART,

ART. VI.

Observations on Thunder and Electricity, by
 EBENEZER M'FAIT M. D.

THE experiment proposed by Mr. *Franklin*, to prove that lightning and the electrical fire are the same, has often been repeated with success both in *England* and abroad; so that the most noted electrical experiments have been performed by fire drawn from the clouds.

MR. FRANKLIN also first discovered, that sharp points attract and discharge the electrical matter most copiously; and from thence supposes, that a very sharp pointed rod, fixed to the extremity of the top-mast of a ship, with a wire conducted down from the foot of the rod round one of the shrouds, and over the ship's side into the sea, would silently lead off the electrical fire, and save the ship from thunder in hot countries; and that, by a similar method, buildings might be preserved,

So useful a proposal deserves to be examined: variety of experiments may give hints for new improvements. For this reason the following observations are communicated, tho' not so compleat as might be wished, being the result of one trial only.

It seldom thunders in this northern clime. In *June* 1752, there seemed to be some thunder at a distance from *Edinburgh*; but, from the beginning of *July* to the beginning of *October*, we had nothing almost but continual rains. The last summer was uncommonly warm and dry; and yet we had only a few claps of thunder at *Edinburgh*, one evening: and my attempts for making any of those experiments were entirely unsuccessful till *Saturday's* night *September* 15. when we had a very great storm.

I used a round iron rod, two tenths of an inch diameter, about eleven feet long, sharpened at one end; the other end was inserted into a glass-tube; and that tube stood in a common glass-bottle, which I held in my hand.

I used also another rod about three feet long, sharpened in like manner, at one end; which stood with the other end in a glass-tube,

tube, which was stuck in the ground. I began upon the *Calton-hill*.

THE lightning and fire in the air abounded greatly ; and yet it was some time before any thing else appeared. At last, some rain began to fall, and the air turned moister ; then fire appeared upon the extremities of each of the rods in a small pretty blaze : very like the fire which is discharged from the point of a sword in the dark, when the person that holds it is electrified, and stands upon glass or rosin ; or like that which appears upon any sharp point, when presented to an electrified gun-barrel, but in greater quantity. I touched the long rod with my finger ; but had no sparks from it. The short rod was accidentally taken out of its tube, and yet continued to burn and blaze as formerly. In like manner the flame continued upon the end of the long rod, tho' I took hold of it any where at pleasure above the glasses, till I moved my hand or finger along, within a few inches of the flame ; then it was attracted by my hand, and vanished.

I WENT from the *Calton-hill* to the *Castle-bill*, at the other end of the town ; and, in passing thro' the streets, no fire appeared up-
on

on either of the rods : but almost immediately when I got clear of the houses, upon the open hill, the point of the longer iron rod took fire. In the dark, I had lost the tube belonging to the shorter rod ; and the point of it did not catch fire when the longer one was kindled. Perhaps I did not wait long enough for a proper trial ; for I soon touched the flame upon the long rod with the sharp point of the short one, and then it also took flame and continued burning, as before, without any further dependence upon the longer one.

I HELD the shorter rod by the sharp end, and approached the blunt end of it to the flame, upon the point of the longer rod ; then this blunt end caught the fire, and the flame upon the points of the two rods continued rather stronger, than on the single one before, so long as I kept them in contact, and the fires within three or four inches of one another : but, when I drew them farther asunder, the flame upon the extremity of the blunt rod vanished. This happened as often as I tried it ; and it is evident, that in like manner, I could have got the fire to fix upon the points of a great many rods, and
so

so have had them all flaming together. Once or twice, a flash of lightning seemed to dart directly against the point of the rod; then the fire, as I thought, expanded itself and united with the lightning; but it immediately began to shine again, when the lightning was past.

THO' it rained much in time of these observations, yet the fire upon the ends of the rods did not go out until it became so heavy, as if it were pouring down out of funnels.

AFTER this I went home for some time, resolving to come abroad again when the storm was more tolerable; but it continued to rain all night, so violently, that I was obliged, with regret, to leave several experiments to the chance of some future opportunity.

FOR example, I suspected that the glass-tubes had not been of great use on this occasion; and wanted to have tried, whether I should have had the same appearances by using the rods alone, without any other *apparatus*. This is very probable; as also, that the glasses, by being wet, allowed the electrical fire to flow off as it was attracted.

I BEG leave to add a few remarks relative to this subject.

IT would seem that experiments of this kind may be made without danger, when the thunder is at a moderate distance.

THE lightning expands itself, as it flies ; and, by expansion, loses its vigor.

PERHAPS there is one simple and easy way of protecting masts and spires from thunder, *viz.* to fix horizontally, upon the highest parts of them, a flat round piece of wood, of a foot diameter, or more, in order to prevent those blazing fires from fixing upon them, and accumulating.

THIS storm passed directly over *Edinburgh*, and came on from the south by west, as nearly as could be estimated. There was a great deal of lightning, that night, above sixty miles to the westward ; but no thunder heard. At *Glasgow*, there was very much lightning, and a few distant faint claps of thunder. On the road from *Belford* or *Berwick*, it lightened incessantly ; but two claps of thunder only were heard, and those very faint : so that there is reason to think, that the fire of this storm spread over the breadth of 130 miles, at least. I wish I could also give some account where this thunder began, and how far it run before it was extinguished.

On

ON *September* 3. there were a great deal of streamers, which rose nearly from the same point that the thunder afterwards came from ; and gradually worked north, till they descended below the horizon. The air had a thunder-like appearance, for several days before this storm ; and for some nights after it, the streamery vapour appeared equally diffused, muddy, inert and languid, and not vibrating any variety of colours ; as if the more volatile parts had been consumed. It is highly probable that lightning and the *aurora borealis* are of the same materials. In hot countries streamers are not seen, or but rarely ; because they are kindled into thunder and flashes of lightning : in cold countries streamers abound, and it seldom thunders. The streamers have served to predict thunder to follow next day, in summer ; and they have been also seen to break out into flashes of lightning. Thunder disturbs the motion of the magnetic needle ; and it has been lately found in *Sweden*, that streamers do the same. Thus thunder, electricity, magnetism, and the *aurora borealis*, appear all wonderfully related ; and many things

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things remain undiscovered in this vast field, which is but just newly opened.

As it is probable that the height which some philosophers have assigned for the streamers in the atmosphere, is by several hundreds of miles too much; it were to be wished that people in various latitudes would carefully observe their altitude, at different times of the night; that, by comparing simultaneous observations, this matter may be determined with more certainty.

ART.

ART. VII.

*Some Phænomena observable in foggy Weather ;
by the same.*

ONE may often meet with opportunities of making observations : but he may not always have leisure enough to judge of their importance, and make a proper use of them. This is the case at present, with respect to what follows.

I HAVE frequently observed, in foggy weather, during frost, that, when the sun was come to a proper altitude, there was a bow formed in the mist, of a white colour ; and the altitude of the bow seemed to subtend an angle equal to the elevation of the sun above the horizon, and formed a semicircle.

THIS kind of mist comes from the ocean, with a slow north-easterly wind ; and overspreads all the lower grounds : but, in a country like ours, so much diversified with long ridges of hills, and interjacent plains and valleys, one may often emerge out of the
mist

mist by going up some neighbouring hill ; then he will sometimes see the whole country around, as it were, buried under a vast deluge ; and nothing but the tops of distant hills, appearing here and there above the flood ; and he will think of diving down again into it, with a kind of horror. The air at that time, in these regions, is very pure, serene, and agreeable to breathe in.

IN this, or a similar situation, there is a phenomenon observable, which I have seen many years ago ; namely, a rainbow formed round one's shadow in the mist. The *French* Gentlemen who went to *Peru*, with a view to determine the figure of the earth ; having, among many other difficulties, the embarrassment of tedious and very frequent fogs to struggle with ; had often occasion to observe this phenomenon. It seemed also worth mentioning here, that any one who has the curiosity may satisfy himself concerning it.

I HAD occasion to see it again on the 23d of *October* last, in travelling from *Glasgow*, northward. When I had ascended a very little out of the fog, so that my shadow was projected amongst it ; the sun being behind, and almost on a level with me ; there was a
double

double range of colours, like those of the rainbow, formed round the shadow. The colours of the outermost range were broad and very distinct, and about two feet distant from the shadow every where ; then there was a darkish interval, and after that another narrower range of colours, closely surrounding the shadow, which was very much contracted.

It is remarkable, that there is a ring of light, brighter than ordinary, that surrounds the shadows of all bodies ; occasioned by the inflexion of the rays of light, as they pass by the surfaces of those bodies. But I dare not proceed to offer my conjectures in what manner this inflexion of the rays contributes to the formation of those ranges of colours.



ART.

ART. VIII.

Of the Measures of Scotland, compared with those of England, by JAMES GRAY.

THE standard pint jug in the custody of the burgh of *Sterling*, is made of brass, in form of a *frustum* of a cone; it weighs near sixteen *Averdupois* pounds. It appears, by its make, to be very old, and has two shields in relievo upon its side, with rude figures upon them, as represented in the margin. Its dimensions, as near as could be taken from a vessel of such rude workmanship, are, *viz.*



Mean

	<i>Inches.</i>
Mean diameter of its mouth,	$4\frac{3}{4}$
Mean diameter of the bottom,	$5\frac{3}{4}$
Mean depth,	6

It was five times carefully filled with clear river water, which was each time accurately weighed by a nice beam, that would turn, with less than a grain, when 100 ounces were in each scale; and its content of water, at a medium of these trials, which did differ but a few grains from one another, was found to weigh 26180 *troy* grains.

At the same time, a vessel which was made to contain accurately, one hundred cubic inches, was filled with the same water: and after several trials, which did not differ one grain from one another; its content in water was found to weigh 25318 *troy* grains.

The standard pint jug therefore contains $103\frac{4}{1000}$ cubic inches; and each cubic inch of water with which the jug was filled, weighs $253\frac{18}{1000}$ *troy* grains.

The standard *Sterling* jug, in the custody of the Dean of Gild of *Edinburgh*, likewise made of brass; and weighing about $20\frac{1}{2}$ *A-verdupois* pounds, having the arms of *Scotland* and of *Sterling* marked in *relievo* upon

C c

its

its side, was in the same manner filled with water; and its content was found not to differ from that in the custody of the burgh of *Sterling*, above a fiftieth part of a cubic inch. Perhaps it may not be improper to observe, that notwithstanding the nicety of the beam, and the small surface of the mouth of the vessel, which contained 100 cubic inches; no difference was perceptible in the weight of its content of water, tho' the heat of this fluid was several degrees both above and below temperate in the thermometer; and that no difference was to be found between spring and river water; nor even between river water when clear, and when somewhat discoloured after a speat.

By act. parl. of 19. *February* 1618. anent settling the measures and weights of *Scotland*, it is statute and ordained, that the wheat firloot shall contain $21\frac{1}{4}$ of the *Sterling* jug; and that the bear firloot shall contain 31 of the same.

THE *English* wine gallon contains, according to the Guild-hall standard, 224 cubic inches; but, according to statute, 231 cubic inches. The *English* ale gallon, both according to the Exchequer standard and statute, contains 282 cubic inches. The *Winchester*

chester bushel, according to act of parliament, 1697, and ratified in the first year of Q. Anne, contains $2150\frac{4}{1000}$ cubic inches. Therefore

Cubic Inches.

The <i>Scotch</i> pint contains	-	$103\frac{4}{1000}$
The <i>English</i> wine pint, according to statute, contains	-	$28\frac{7}{8}$
The <i>English</i> ale pint contains	-	$35\frac{1}{4}$
The wheat firloft contains	-	$2197\frac{3}{1000}$
The bear firloft contains	-	$3205\frac{5}{1000}$
The <i>Winchester</i> bushel contains		$2150\frac{4}{1000}$

ACCORDING to act parl. of 19th February 1618 above mentioned, the *Scotch* pint jug contains, of the clear running water of *Leith*, three pounds and seven ounces of *French Troas* weight, ordained to be the weight of *Scotland*; whence the *Scotch* pound is equal to 7616 *Troy* grains, or $15\frac{1}{12}$ ounces *Troy*.

By an accurate comparison of standard weights made at *London*, (*vid. Philosoph. Transact.* N° 470.) the *Averdupois* pound was found equal to 7000 *Troy* grains. Therefore

The

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	<i>Troy Grains.</i>
THE <i>Troy</i> ounce being equal to	480
The <i>Scotch</i> ounce is equal to	476
The <i>Averdupois</i> ounce is	437 $\frac{1}{2}$

	<i>English Inches.</i>
THE <i>Scotch</i> foot is	12 $\frac{1}{5}$
The <i>English</i> foot is	12
The <i>Scotch</i> ell, according to the standard of <i>Edinburgh</i> , is equal to	37 $\frac{2}{5}$
The <i>English</i> ell is	45
The <i>English</i> yard is	36

	<i>English Feet.</i>
THE <i>Scotch</i> mile contains	5952
The <i>English</i> mile contains	5280
The <i>Scotch</i> acre	55353 $\frac{6}{10}$ square.
The <i>English</i> acre contains	43360 square.

ART.

ART. IX.

A Dissertation on the Sexes of Plants ; by CHARLES ALSTON, M. D. King's Botanist in Scotland, Fellow of the Royal College of Physicians, and Professor of Medicine and Botany in the University of Edinburgh.

THE smallest, and apparently most despicable productions of nature, are not so barren, but that they are capable, both to invite our speculations, and to recompence them. Even *Pliny* could say, “*rerum natura nusquam magis quam in minimis tota est*”. (a) For how much soever the celestial bodies declare magnificence, and immensity of power ; yet do they not by far afford us so clear and cogent arguments of wisdom and design, as do the bodies of animals and plants. And for my part, says the great *Boyle*, I am apt to think, that the eye of a fly is a more curious piece of workmanship than the body of

(a) L. xi. c. 2;

of the sun, at least as far as appears to us (*b*).
Hence

THE exquisite structure of flowers, especially of the most minute, and scarcely visible dust of the *apices*, commonly the sport of the winds, has engaged the attention of the learned, both of this and the preceeding age: and yet, no body has been able to determine, with any certainty, whether it is absolutely necessary to the fecundity of the seeds, or excrementitious only; which is the subject of the following inquiry.

ANIMALS and vegetables resemble one another in so many things, that not only some of the antients, but even of the moderns also, seem to be at a loss how to distinguish them, or fix the limits of each of these kingdoms (*c*); yea, and to reckon analogy a sufficient proof, that such properties as are generally observable in the one, do belong to the other also. Thus *Empedocles* and *Anaxagoras* seem to have reasoned, when they maintained, that both sexes were conjoined in plants; that they were animated, yea and sentient

(*b*) Vid. BOYLE's works, fol. edit. vol. I. p. 428. & vol. iv. p. 523.

(*c*) Vid. TYROCIN bot. p. 3.

tient beings, capable of pleasure and pain, desire and aversion, &c. (*d*)

I SHALL follow the philosophers no further ; my design being only to inquire into the rise and progress of the doctrine concerning the sexes of vegetables among botanists ;
and

(*d*) “ Vita et in animalibus et in plantis esse deprehensa
“ est. ANAXAGORAS itaque et EMPEDOCLES desiderio
“ plantas duci aiunt ; sentire item, ac tristitia voluptateque
“ affici affirmant. Et ANAXAGORAS quidem, animalia i-
“ psas esse, et voluptate ac dolore moveri, docuit ; e foliorum
“ scilicet defluvio, et ex incremento, istud colligens. EM-
“ PEDOCLES vero sexum his admistum esse arbitratus est. Eo-
“ dem modo PLATO quoque appetitu solum illas duci,
“ ob vehementem scilicet facultatis altricis necessitatem, affir-
“ mabat Quod si constet plantas voluptate ac dolore affici,
“ tum sentire quoque rationi erit consentaneum : et ubi hoc
“ constiterit, tum appetitu quoque duci ; si quidem somno
“ reficiuntur, et vigiliis excitantur, rationi consentaneum erit.
“ Ad eundem modum si quaeramus, an spiritum ducant, et
“ sexuum missionem habeant, an contra sit ; multam super
“ hoc ambiguitatem, et quaestionem prolixam excitabimus.—
“ Cum in plantis reperiatur, quod unaquaeque species mas-
“ culum genus habeat et femellum, et omnino quod mascu-
“ lum est asperius ac durius rigidiusque, femellum debilius
“ et foecundius ; quaerendum rursus est, inveniaturne haec
“ duo genera simul commista in plantis esse, ut EMPEDOCLES
“ dicit. Id quod ego sane ita habere non arbitror ” Thus
ARISTOTLE de plantis, lib. 1. c. 1. et 2. See also Frag-
mentum GALENI in PLATONIS TIMAEUM, c. 2. Et li-
brum de historia philosophica GALENO adscriptum sub fi-
nem.

and whether it is supportalbe by experiments, or a mere hypothesis?

SECT. I.

THEOPHRASTUS, the greatest, as well as the eldest, of the *Greek* botanists whose works have been preserved, can best explain, why some plants of the same kind were called females, and others males, by them. I know some reckon *Crateva* more antient, and contemporary to *Hippocrates*, on the authority of some epistles fathered on the old man, and annexed to his works. But the learned have proven these letters to be spurious; and that *Crateva* lived not before *Mithridates*, whom he complimented with the name of a plant, as *Pliny* (e) informs us. As for *Hippocrates* himself, I find in him a *conyza femina*, but no other plant called either male or female.

2. But *Theophrastus*, who succeeded *Aristotle* in his school, in the 114. olympiad, very frequently mentions the sexes of plants. Thus *φασι δὲ τινες*, says he, καὶ τῶν ὁμοιογενῶν, τὰ μὲν ἀρρεῖν, τὰ δὲ ὄν, &c. “But it is said, that
“of plants of the same kind, some flower,
“others

(e) lib. 25. c. 6.

"thers not; as of the *palm-trees*, that the
 "male blossoms, not the female, which a-
 "bout the same time puts out the fruit.
 "Plants therefore of the same kind, are thus
 "differenced, as also all such as cannot perfect
 "the fruit." (f) And elsewhere, (g) "the
 "most common distinction of trees, is into
 "female and male, whereof the one is fer-
 "tile, but the other barren, in some. But,
 "when both are fertile, the female carries
 "the fairest fruit, unless such be called males,
 "for so some call them." And almost all the
 plants which he distinguishes into male and
 female, are equally fertile in both sexes, as
abies, filix, cornus, tilia, cupressus, cistus,
conyza, &c. Even *palm-trees* he divides in-
 to fructiferous and barren; and the fructife-
 rous again into female and male (b): so seems
 not much to have regarded analogy, in the
 distinction of sexes, except it be in one species]
 of the *palm-tree*.

D d

3. BUT

(f) Hist. lib. 1. c. 22.

(g) Hist. lib. 3. c. 9.

(b) Hist. lib. 2. c. 8.

3. BUT of this famous tree he says, (i) unless the *spatha* be cut from the male, and, while it retains the down, flowers and dust, (k) be shaken over the fruit of the female; it will never ripen, but fall off: which this sprinkling prevents. "For the male, adds "he, of both the *fig-tree* and the *palm-tree*, "is of use to the female, ἀλλ' ἡ μὲν οἷον μίξις, "ἡ δὲ κατ' ἄλλον τρόπον, but in the one (l) there "is, *quasi coitus*; in the other it happens after a different manner." And elsewhere, "(m) That the fruit will not continue on the "female *palm-tree*, unless the flower of the "male with the *κονιορτος* be shaken over it, "as some affirm, is certainly singular: for "tho' there is an evident reason for caprification, which it somewhat resembles; yet "none can be assigned for the effect of this "sprinkling." And least it should hence be inferred in general, that female trees were of themselves insufficient for fructification, without the assistance of the males; he observes, that in order to such a conclusion, this ought

(i) Hist. lib. 2. c. 9.

(k) Κονιορτός.

(l) *viz.* Palm-tree.

(m) Caus. Plant. lib. 3. c. 23.

ought to appear, not in one or two instances (*n*), but in all, or the greatest part of the females, τὴν γὰρ φύσιν ἔτω κρείνομεν τῷ γένει. Of this tree more afterwards.

4. DIOSCORIDES, who lived under Nero, has a male and female *mandragora*, *abrotonum*, *mercurialis*, *anagallis*, *aristolocchia*, *cistus*, *felix*, *pæonia*, *polygonum*, *tithymalus*, *verbascum*, and an *arundo fœmina*: but, without any regard to analogy, or to their fertility or barrenness, does he call them so; for his *mercurialis*-mas carries the seed, and the *fœmina* is barren; and the male of all the rest, equally fertile with the female. The same may be said of *Galen* and the succeeding *Greeks*; none of them, so far as I have observed, mentioning the sexes of the *palm-tree*.

5. PLINY, who flourished under *Vespasian*, says indeed, “ Arboribus, imo potius
 “ omnibus quae terra gignit, herbisque etiam,
 “ utrumque sexum esse, diligentissimi naturae
 “ tradunt: nullis tamen arboribus (palma)
 “ manifestius. Mas in palmite floret, fœ-
 “ mina citra florem germinat tantum, spicae
 “ modo.” (*o*) But when he comes to mention
 particular

(*n*) As here in the fig-tree and palm-tree.

(*o*) L. 13. c. 4.

particular plants, excepting what he has from *Theophrastus* of the *palm-tree*, he transgresses analogy as much as *Dioscorides*: his *mercurialis masculus* being the seed-bearing one ; and his other male and female plants the same with those of *Theophrastus* and *Dioscorides*. Hence it appears that the *palm-tree* is the only instance, among the antients, where sexes are attributed to particular plants, on account of fertility or barrenness : other plants being called male or female, on various and very different accounts, and for distinctions sake only.

6. In a word, from *Theophrastus* down to *Cæsalpinus*, who died at *Rome* in 1603, the analogy between plants and animals, with regard to sexes, seems to have been intirely neglected. But *Cæsalpinus* (*p*) says exp्रेसly, “ *Fructum ferunt, non vero florent oxycæ-*
drus, taxus ; in genere herbaceo mercuria-
lis, urtica, cannabis : quorum omnium ste-
riles mares vocant, faeminas autem fru-
ctíferas. In eo tamen genere faeminas
melius provenire et foecundiores fieri aiunt,
si juxta mares serantur ; ut in palma est a-
nimadversum :

(*p*) De plantis, l. 1. c. 3.

“nimadversum: quasi halitus quidam, ex
 “mare efflans, debilem faeminae calorem ex-
 “pleat ad fructificandum.”

SECT. II.

7. BUT of late it has been maintained by not a few, that there is as real a diversity of sexes in every species of plants, as in every species of animals; and, first of all by the celebrated naturalist Dr. *N. Grew*, to whom therefore the honour of the invention is of right due. For this great man, in his anatomy of flowers, read before the Royal Society *November 9. 1676*, after noticing the secondary uses of the dust of the *apices*, which he calls *globulets* or small particles within the *thecæ* of the attire; he adds, “But the
 “primary and chief use is such as has respect
 “to the plant itself, and so appears to be ve-
 “ry great and necessary; because even those
 “plants which have no flower or foliature,
 “are yet some way or other attired; so
 “that it seems to perform its service to the
 “seed, as the foliature to the fruit. In dis-
 “course hereof with our learned *Savilian* pro-
 “fessor, Sir *Thomas Millington*, he told me,
 “he

“ he conceived that the attire doth serve as
 “ the male for the generation of the seed.
 “ I immediately, replied, that I was of the
 “ same opinion ; gave him some reasons for
 “ it, and answered some objections that might
 “ oppose them.”

WHETHER, as some pretend, this compliment paid to Sir *Thomas Millington*, shows him to have been of this opinion before our author, I leave to the learned to determine ; for my part, I think the words cannot bear it. And Mr. *Ray* (q) gives it for Dr. *Grew*, without naming Sir *Thomas*.

8. DR. GREW then proceeds to give the sum of his thoughts concerning this matter ; and plainly asserts, as his opinion, that when the attire or *apices* break, or open, the globules or dust falls down on the seed-case or *uterus*, and touches it with a *prolific virtue* ; not by entering bodily, or as to its gross substance, but only by communicating to it some *subtle and vivific effluvia* (r).

9. AMONG the first who adopted this doctrine, was Mr. *John Ray*, that great natural historian ; at first indeed only as probable
 (s),

(q) Hist. p. 17.

(r) See GREW's anat. fol. p. 171.

(s), but afterwards as proven by many arguments, which are collected in the preface to his *sylloge stirpium Europearum extra Britannias nascentium*, printed at London 1694, in 8vo: whether these arguments sufficiently prove the doctrine will be considered below.

10. In 1695, *Rudolphus Jacobus Camerarius*, professor of botany and medicine at *Tubingen*, published there an *epistola de sexu plantarum*, in 12mo (t): this I have not seen, but only an abstract of it in the appendix to *Miscel. nat. cur. Dec. 3. an. 3.* for 1695, and 1696, by *M. B. Valentini*. Here the generation of plants to be analogous to that of animals, he endeavours to shew in eight particulars; the last of which he calls the *palmarium* and *ponderosissimum argumentum*: and it is this, *Certum est, says he, ad animalium generationem copulam utriusque sexus exigi: quae in plantis adeo quoque necessaria est, ut si vel maris apices, vel faeminarum styli, vel utraque deficiant, nulla proles sequi possit; ut in frumento turcico, cui juba praematurè ressecatur, et mercuriali mare, a faemina separata, constat.* But then he proposes
three

(s) Vid. R. Hist. p. 18.

(t) Reprinted *Frankfurti*, 1701, in 4to *Lin. bib. bot.* 173-

three objections against his own doctrine, which are stronger than all his arguments for it; yea, the third plainly confutes it: and they are these, as *Valentini* delivers them; “*1mo*. Plantae dantur apicibus copiosis, aſt
 “ nullo ſtylo praeditae; adeoque mares ſine
 “ faeminis, ut in equiſeto, lycopodio, &c.
 “ *2do* Videtur e contrario ſibi obſervaffe faemi-
 “ ninum ſine pretenſo virili ſemine. *3tio*, Mer-
 “ curialis, ſpinachiae, et cannabis femellas ſo-
 “ litariò, abſque vicini maris contagio excul-
 “ tas, plurima granula ſeminaque foecunda
 “ obtinuiſſe vidit: et haec in cauſa erant, cur
 “ haec omnia cum aliquali oppoſiti formidine
 “ proponat.”

11. IN 1703, Mr. *Samuel Morland* laid before the Royal Society, ſome new obſervations on the parts and uſe of the flower in plants (*u*); here he ſays, “ Dr. *Grew* falls ſhort, in that
 “ he ſuppoſes the *farina* only to drop upon
 “ the outſide of the *uterus*, and to impregnate
 “ the included ſeed by ſome *ſpirituſous ema-*
 “ *nations*, or *energetical impreſs*.” So he pa-
 “ raphraſes *proliſic virtue* and *ſubſ'e vivific efflu-*
 “ *via*. He then propoſes a more probable hy-
 “ potheſis, as he thinks, viz. “ that the *farina*
 “ is

(*u*) *Philof. Tranſact.* No 287.

“ is a congeries of seminal plants ; one of
 “ which must be conveyed into every *ovum*
 “ or seed, before it can become prolific.” He
 has the honour, I believe, of being the in-
 ventor of this hypothesis ; but is so far from
 making it probable, that the structure and po-
 sition of the parts of some of the flowers he
 instances, which are all the arguments ad-
 vanced by him for it, sufficiently confute it ;
 e. g. *corona imperialis*. For allowing for
 once, which however cannot be allowed, that
 the *stylus* is always hollow, and that rain
 might wash the dust, or wind shake it down
 this cavity, into the heart of the seed in erect
 flowers ; what must become of pendulous
 flowers, such as the *crown imperial*? here the
farina must be washed or shaken upwards.

12. As for *Waldschmidii dissertatio de sexu
 plantarum*, Kiliae 1705, 4to, and *Gakenholzii
 dissertatio de vegetabilium indole cognoscenda*,
Helmstadii 1706, in 4to, mentioned by the
 learned *Heisterus*, in his *praefatio in epistolam
 Burckhardi, Helmstadii* 1750, in 8vo ; I know
 nothing about them : but I have no reason
 to think, that they have been more successful
 in demonstrating the sexes of plants than *Ca-
 merarius*, “ qui, omnium planissime, sexum
 E e “ plantarum

“plantarum exposuit;” as the same professor *Heister* (x) testifies.

13. IN 1711, Mr. *Geoffroy* presented the Royal Academy at *Paris*, with “observati-
“ons on the structure and use of the princi-
“pal parts of flowers;” wherein he attempts to prove, that the dust of the *apices* really impregnates the seed. And after advancing almost all that had been said on this subject by *Grew*, *Ray*, *Camerarius*, *Morland*, without mentioning them; he asserts, 1mo, That the germ is never to be seen in the seed till the *apices* shed their dust. And, 2do, That, if the *stamina* be cut out before the *apices* open, the seed will either not ripen, or be barren if it ripens: in proof whereof he gives two experiments that he made, one on the *mays*, by cutting off all the *stamina* before the *apices* opened; and the other on the *mercurialis*, by training up separately some of the seed-bearing plants by themselves. The consequence was, some of the ears of the *mays* aborted altogether; others ripened a few seeds; as did also the *mercurialis* plants: both which seemed to be *foecund*. “Perhaps, says he, “the dust of the *apices*, brought from some
“other

(x) Pref. p. 70.

" other place by the wind, fertilized these few
 " seeds : that this is not impossible, we have
 " a fine instance in *Jovianus Pontanus*, precep-
 " tor to *Alphonfus* King of *Naples*, who, in a
 " poem, tells us, that in his time, there were
 " two *palm-trees*, a male cultivated at *Brindes*,
 " and a female in the woods of *Otranto*, which
 " carried no fruit for many years. But at
 " length being elevated above the other trees
 " of the forest, so that it could see the male
 " *palm-tree* of *Brundisium*, it then began to
 " bear good fruit in abundance. No doubt,
 " because then it began to receive the dust
 " of the *flamina*, carried by the wind from
 " the male *palm-tree*, over the other trees of
 " the wood (y)." A very fine story indeed !

I shall only further observe, concerning
 this learned author, that tho' he has added
 nothing new, of consequence, on this subject,
 yet he seems to claim the honour of being
 the inventor of all ; for after giving his con-
 jectures, with relation to the manner how
 this dust impregnates the seed, and as his
 own too, tho' the one be *GREW's* and the o-
 ther *Morland's*, he thus concludes,

" BUT

(y) Vid. Mem. Acad. 1711.

“ BUT whichsoever of these conjectures
 “ be pitched on, it remains always certain,
 “ *by my observations*, that the dust of the *a-*
 “ *pices*, which has been hitherto neglected,
 “ as vile excrements, which in a manner
 “ disfigured the flowers, are nevertheless parts
 “ essential, and necessary to the fecundity of
 “ plants.” The pains however he has been
 at, in examining and describing the dust of
 the *apices*, of a considerable number of plants,
 if accurately performed, might have procured
 him better treatment, than he met with from
 some of his countrymen.

14. IN 1717, M. *Vaillant*, at the opening
 of the Royal garden, entertained his audience
 with a discourse on the structure of flowers,
 and the use of their parts ; which was print-
 ed in *French* and *Latin* at *Leiden* in 1718, in
 4to : and thus he begins, “ Since the flowers
 “ are, without controversy, the most essential
 “ parts of plants, it is very fit that I entertain
 “ you, in the first place, with them ; and
 “ the rather because every botanist hitherto,
 “ has given only confused ideas of them. The
 “ language perhaps which I am to make use
 “ of, may appear somewhat new in botany ;
 “ but, as it will abound in terms altogether
 suitable

" suitable to the use of the parts which I am
 " to explain ; I believe it will be much bet-
 " ter understood, than the old one, which
 " being stuffed with improper and equivocal
 " words, more apt to perplex than illustrate
 " the matter, precipitate into error those,
 " whose clouded imagination has no right
 " notion of the functions of these parts. The
 " flowers, strictly speaking, are nothing else
 " but the organs which constitute the differ-
 " ent sexes of plants, &c." I shall notice
 only two or three things concerning this dis-
 course. *1mo*, That, according to it, the *a-*
pices of barren flowers shed their dust all at
 once, by a kind of explosion ; but fertile
 flowers, slowly and by degrees, and common-
 ly before they open, or expand their covers ;
 but he gives only the *parietaria* for an in-
 stance.

2do, THAT he has demonstrated that the
 dust of the *apices* cannot enter the seeds ; be-
 cause the *stylus* is not always hollow, but of-
 ten solid : and, although it were hollow or tu-
 bular into the seed-vessel, it could not thus
 convey the dust into the seeds, without pe-
 netrating their proper covers or shells. Be-
 sides, he asserts, and that truly too, that this
 dust

dust does not enter even the cavity of the fruit; as may be seen by opening the heads of the *papaver orientale hirsutissimum flore magno*, T. Cor. p. 17. when the flower is fully blown. For, tho' the upper part of the heads, are then covered with the purple dust of the *apices*; yet the seeds, partitions to which they adhere, and all within the fruit, continue perfectly white. I might add, that the dust of the *apices* is sometimes in so large grains, as to be visible to the naked eye: as in some of the *malvaceae*, while no conduits are discoverable, by magnifying glasses, in the *stylus*, whose diameter does not much exceed that of the grains of the dust, which resembling prickly balls, must be very unfit to enter a strait passage. And,

3tio, THAT the volatile spirit, or vapour of the dust, may be conveyed to the seed, in this manner; it may enter the air vessels of the *stylus*, pass through them into the *placenta*, thence into the *funes umbilicales*, and so into the *ova* or seeds, along with the nutritive juices. But I cannot omit a remarkable passage in this discourse, as it answers an assertion of Mr. Geoffroy, and on other accounts which take in the translators words.

“ Tubae

“ Tubae igitur hae, quas fallopianis compa-
 “ ro, quod ad ova deferant, non exigua ipsa
 “ illa pulveris foecundi grana, quae apices su-
 “ per illas ejaculantur, aut in ipsarum excu-
 “ tiunt infundibulum, ut sectator *Leeuwen-*
 “ *hoekianorum* atque *Hartfoekerianorum* phan-
 “ tasmatum voluit; sed halitum modo, aut
 “ spiritum volatilem, qui pulvere hoc se ex-
 “ pedit, ovaque ipsa foecundat. Credo enim
 “ auditores, persuasum certumque habendum,
 “ non materiam masculinam, nec vermiculos
 “ supposititios, vel animalcula seminalia esse,
 “ quae impregnationem in femella absolvant:
 “ quia *Malpighius*, narrante anatomico recen-
 “ te (x) agnovit foetum reperiri in ovis rana-
 “ rum et gallinarum ante copulam: ut et
 “ certissimum est germen adeste in seminibus
 “ plantarum quae non fuerunt impregnata,
 “ quorumque parenchyma facit cum germi-
 “ ne ipso continuum corpus. Non poterit
 “ ideo esse aliud quid, preter volatilem hunc
 “ spiritum, cui crassior materies vehiculi mo-
 “ do vicem praestat simplicis. Natura vero
 “ semper easdem sectante leges, concludere
 “ oportet, id quod hac occasione in anima-
 “ libus

(x) M. DIONIS edit. 1715. p. 392.

“libus contingit, idem et vegetantibus acci-
 dere (a).” Upon the whole, it is evident
 that Mr. *Vaillant* assumes more the air of an
 original writer on this subject than belongs to
 him. He never mentions *Grew*, *Ray*, or
Camerarius; and quotes only some passages
 from *Geoffroy*, for the pleasure of confu-
 ting them.

15. I shall pass a variety of later authors
 who have treated on this subject; and come
 to the most strenuous defender of the sexes of
 plants, who has collected all the arguments
 for it that perhaps can be advanced, and pre-
 tends to have demonstrated it fully: I mean
 the famous and very learned *Carolus Linneus*,
 professor of medicine and botany in the uni-
 versity of *Upsal*, fellow of a great many phi-
 losophical societies; and certainly one of the
 greatest botanists of this age. For this great
 man thus writes, “Antheras et stigmata (b)
 “constituere sexum plantarum, a palmicolis,
 “*Millingtono, Grewio, Rayo, Camerario, Go-*
 “*dofredo, Morlando, Vaillantio, Blairio, Jus-*
 “*sievio, Bradleyo, Royeno, Logano, &c. de-*
 “tectum,

(a) Vid. p. 17.

(b) that is, the apices, and extremity of the stylus.

“ detectum, descriptum, et pro infallibili as-
 “ sumptum: nec ullum, apertis oculis confi-
 “ derantem cujuscunque plantae flores, latere
 “ potest; quod demonstratum in *sponsalibus*
 “ *plantarum*, Upsaliae 1746, in 4to (c).” And
 elsewhere (d) “ Generationem vegetabilium
 “ fieri, mediante pollinis antherarum illapsu
 “ supra stigmata nuda, quo rumpitur pollen,
 “ efflatque *auram seminalem*, quae absorbetur
 “ ab humore stigmatis; quod confirmat ocu-
 “ lus, proportio, locus, tempus, pluviae, pal-
 “ micolae, flores nutantes, submersi, synge-
 “ nesia; immo omnium florum genuina con-
 “ sideratio.”

YET I cannot help thinking this doctrine
 not capable of demonstration, far less that
 the *genuina consideratio* of any flower can
 make it probable: *Camerarius* himself doubt-
 ed of it; *Tournefort* disbelieved it; and *Pon-*
tedera (e) uses many arguments to refute it.
 It remains therefore only, that the argu-
 ments for and against the sexes of plants, as
 understood by the moderns, especially the de-

F f

servedly

(c) *Systema naturae*, edit. Lipsiae 1748. in 8vo, p. 216.

(d) *Philosoph. Botan.* edit. Stockholm. 1751. in 8vo, p. 91.

(e) *Anthol.* l. 2, p. 107—185.

servedly much commended *Carolus Linnaeus*, be fairly stated.

SECT. III.

16. THE state of the controversy is briefly this ; Whether the influence of the dust of the *apices*, is absolutely necessary to the fecundity of the seeds, or not : or, Whether good and fertile seeds can be produced, when the dust of the *apices* has no access to the *styli* or *stigmata* of the plants that carry them. Now, there being several species of vegetables, which bear flowers on one plant, and seeds on another, as *spinacia*, *mercurialis*, *cannabis*, &c.; I know no way, to determine the question so certainly, as by training up one or more of these seed-bearing plants, at a sufficient distance from such as carry the flowers, and observing the consequence : for,

17. If a seed-bearing or female *spinacia*, *mercurialis*, or *cannabis*, standing at a distance from any of the flowering or male plants, can produce fertile seeds ; then the influence of their dust is not necessary to the fertility of these seeds : but, “ foemellam spinaciae, mercurialis, cannabis, absque vici-
“ ni

“ni maris contagio excultam, semina foe-
 “cunda producere vidit *Camerarius* ;” ergo
 the dust of the *apices* is not necessary to the
 foecundity of all seeds. Although this testi-
 mony of *Camerarius* is beyond all exception ;
 yet, in confirmation of it, and to prevent ca-
 villing, I shall relate the experiments I made
 with the same plants.

1. In the Spring 1737, I transplanted
 three sets of the common *spinage*, long be-
 fore it could be known, whether they were
 flowering or seed-bearing plants, from a little
 bed on which they were raised, into a place
 of the garden, full 80 yards distant, and al-
 most directly South ; there being two *baw-*
thorn and three *holly* hedges, all pretty thick
 and tall, between them and their seed-bed ;
 and no other *spinage* in the garden, nor so
 near them by far : all the three proved fer-
 tile plants, and ripened plenty of seeds. I
 sowed them, they grew, and prospered as well
 as any *spinage* seed possibly could do. This,
 I own, made me, at first, call in question the
 sexes of plants, which I formerly too impli-
 citly believed.

2. THE same year, a few plants of the
 common *bemp*, which I had raised for a spe-
 cimen

cimen from the seed, being accidentally destroyed when very young; and finding afterwards, about the end of *June*, a pretty strong but late plant of *bemp*, growing in the inclosure to the east of *Holyrood-house*, commonly called the *Bowling-green*, by itself: I caused great care to be taken of it; there not being that year any *bemp* raised within a mile of it, that I could find. This plant grew luxuriantly; and, tho' bad weather in the Autumn made me pluck it up a little too soon, yet I got about thirty good seeds from it, which the succeeding Spring produced as thriving male and female plants, as if the mother *bemp* had stood surrounded with males. And,

3. IN the Spring 1741, I carried two young seedling plants of the *French mercury*, long before there was any in flower, from the City Physic-garden, the only place where it was then to be found in this country, to the King's Garden at the Abbey, which are more than 700 yards distant from one another, with many high houses, trees, hedges, and part of a hill between them: and planted one of them in one inclosure, where it was shaded from the sun the greatest part of the day; and the other, in another, 25 yards distant,

stant, exposed to the South and West. Both plants ripened fertile seeds; and the last shed them so plentifully, that it proved a troublesome weed for several years: tho' none of the species was to be found in that garden, for more than twenty years preceeding.

HENCE the dust of the *apices* of these three species of plants, cannot, on any account, be called their *genitura*. And the same may be said of the *lupulus*, according to Mr. *Tournefort's* observation, (*f*) of the *bryonia*, as noticed by Mr. *Miller* (*g*), yea of Mr. *Geoffroy's* *mays* mentioned above.

18. THE learned *Valentini* (*b*) attempts to remove *Camerarius's* doubts two ways. 1.^o Perhaps, says he, there may be found latent *styli* among the *apices* of the *equisetum*: and 2.^o The wind may carry the *pollen feminine*, (or dust of the *apices*) to as great a distance, as the *mercurialis*, *vel frumenti Turcici feminina*, can be separated from their proper males.

THAT the wind could have this effect, even on the *spinage* male dust, is far from being probable: for, to say nothing of the globular

(*f*) Page 69.

(*g*) Gard. Dict. abridg.

(*b*) German. ephemer. l. c.

globular figure or specific gravity of this dust, a north wind only could drive it toward the females, which coming over the *Caltonhill*, must rather have driven it into the ground, or into the intervening hedges. And indeed this supposition is so evidently extravagant, that I find not any notice taken of it, by the most strenuous defenders of the sexual scheme.

19. BUT the other supposition of the latent *styli*, in *equiseto* &c. seems to have given occasion to another as extraordinary, viz. of latent *stamina*, in *cannabis*, *lupuli*, &c. *foemellis* "Accidit interdum, says Mr. *Wahlbom*, "or rather Mr. *Linnaeus*, (i) ut *cannabis* se-
"minifera, unum alterumve ferat florem
"staminiferum, quo nonnullae feminae im-
"pregnari possint; quod *Camerarius* lu-
"sit." Granting this should happen sometimes, and that these Gentlemen have seen it, (which, however, is not here asserted); yet, I may say, that I believe they are the first that ever saw it; and that this is not enough to remove *Camerarius's* doubt, nor to weaken the
argument

(i) *Amaen. Acad.* vol. 1. edit. Lug. Bat. 2749. 8vo.
p. 99.

argument drawn from the above experiments, unless they can say, that the seed-bearing *bemp* carries always one or more such stamiferous flowers; or that the *cannabis* of *Cammerarius*, and the other abovementioned, had really such flowers on it, which they cannot assert. For my part, I can declare, that, tho' I searched very carefully for the seed of the *bemp* plant, (k) I could see nothing like *stamina* on it; neither on the seed-bearing plants of the *spinacia*, *mercurialis*, *bryonia*, either before or since I read Mr. *Wahlbom's Sponsalia Plantarum*.

20. Hence I think the position of that learned botanist *Julius Pontedera*, viz. "A-
 " picum liquorem embryonis foecundationi
 " in omnibus plantis non esse necessarium,"
 to be a demonstrable truth, which neither authorities nor arguments can refute. However to clear up further this controversy, I shall consider the arguments advanced by the most famous sexualists, as they are summed up by the very learned *Carolus Linnaeus*, in his *Fundamenta Botanica*; explained by *Jo. Ges-*
nerus

(k) Exp. 2.

nerus (*l*), and demonstrated, they say, by *Jo. Wahlbom* (*m*), and *Car. Linnaeus* himself (*n*).

21. Generationem vegetabilium fieri, mediante pollinis antherarum, &c. ut supra, N^o 15. *confirmat* (or, as in former editions, *dictat*) 1. *oculus*, F. B. § 145. How the eye shews or confirms the generation of vegetables, to be thus effected, is explained three different ways. For,

1. “ Ipsa autopsia (says *J. Gesner*) hunc
 “ generationis actum edocet. Primus qui
 “ florum connubia vidit, cel. D. Seb. Vaillant
 “ (*o*) tam eleganter describit, ut nemini de
 “ hac re dubium superesse possit. Ita autem
 “ cl. Vaillant, “ Quoties acciderit, ut in ea-
 “ dem stirpe flores gerantur simul, quorum
 “ hi foemina tantum, illi autem masculina
 “ et foemina conjuncta organa cingunt, ar-
 “ rectio tumorque organorum masculinorum
 “ in his tam subito contingit, ut lobuli gem-
 “ mæ

(*l*) *Dissertationes Physicae de Vegetabilibus*, Lugd. Bat. 1743. 8vo conjunctum cum C. Linnaei oratione de necessitate peregrinationum. Explicant elementa botanica Linnaei. Lin. bib. vol. p. 174.

(*m*) In the *Sponsalia Plantarum*, Amaen. Acad. vol. 1. p. 61. ad 109.

(*n*) In *Phil. Botan.*

(*o*) In lib. de structura florum.

" mæ flosculosæ cedant illorum impetui, at-
 " que hincinde semet expandant, mirabili me-
 " hercle velocitate, et extemplo quam vio-
 " lentissime fœcundam explodant, omnem-
 " que uno impetu ejaculentur genituram,
 " diffusa nimirum pulverulenta nûbecula,
 " spargente quaquaversum fœcundationem
 " arvi genitalis. Vix venereus hic ludus ab-
 " solutus est, quin illico florum labia, aut lo-
 " buli, ad se invicem accedant eodem qui-
 " dem, quo a se mutuo recesserant, celeri-
 " tatis impetu, veteremque ita formam sta-
 " tim renouent. Apparatum hunc artificio-
 " sum facile spectare datur in *parietaria*. Sed
 " accedas oportet hora sacra veneri, aurora
 " est quæ favet his congressibus; ubi vero
 " agere renuunt satis opportune, aciculæ a-
 " pice, leniter modo, stimules. In herma-
 " phroditis, ubi duo sexus conjuncti habentur,
 " multum abest ut tanto impetu explo-
 " datur. Plerique enim flores, præcipue
 " nutantes, in quibus pistillum obliquum
 " intra stamina positum habet, actum gene-
 " rationis floribus clausis exercent, dum stig-
 " ma adhuc intra medias antheras contine-
 " tur." Thus Mr. *Gesner*. (p)

G g

22. ALLOW

(p) Diff. p. 86.

22. ALLOW me here to observe, in the first place, that this artificial *apparatus* (q) was seen and described by *John Baubinus*, long before *Vaillant* was born (r), and transcribed both by *Morison* and *Ray* in their histories of plants, tho' *Vaillant* mentions none of them: and, secondly, that this seems to afford a strong argument against the impregnating virtue of the dust of the *apices*. For, according to *Linneus* himself, "*parietariae hermaphroditi*, "*flores duo continentur involucro plano hex-*
aphyllo. Calyx monophyllus, magnitudi-
ne involucris dimidiati. Stamina filamenta
quatuor calyce longiora, illumque expan-
dentia. Faemineus flos unus, inter herma-
phroditos ambos, intra involucrum; cum
calyce ut hermaphroditi (s)." Now, since
 the stamina of the hermaphrodite flowers,
 as well as the *germen* of the female flower
 betwixt

(q) Tout. cette mecanique, VAILL.

(r) "*Parietariae flosculi conferti circa caulem ex foliorum*
alis, floccos coccinei serici imitantur, primulum e nodulo
emicantes: post se stamina ostendunt obscure, ex albo pur-
purascentibus apiculis, involuta, quae si stylo evolvere co-
neris, subsultim excusso pulvere cum impetu, spectaculo
jucundo, se expandunt repanda, in medio seminis rudimen-
tum circumdantia." I. B. 2. p. 976.

(s) Gen. pl. p. 494.

betwixt them, are contained, in one common *involucrum*, and the *apices* do not eject their dust, until, by the straightning, or stretching out of the formerly crooked *stamina*, the common *involucrum* is burst open, and the *apices* raised in the air; since no sooner do they find themselves free, and erected high enough above the *involucrum*, than immediately, by a sort of explosion, all the dust is entirely, with great violence, thrown out; and since the leaves of the *involucrum* close up again, as quickly, and with as much force as they separated, and resume their former figure (t); does not all this shew, that the dust is not by nature designed to impregnate the seed, but to be carried away by the winds, lest it should infect it, by falling on the *stigmata*; and that *autopsia* teaches the quite contrary to what Mr. *Gesner* alledges.

23. "ITA

(t) "Ita quidem, (adds Mr. VAILLANT), ut difficillimum
 " foret credere, flores hosce ullam vim passos esse, nisi vel ipse
 " actum hunc vidisset oculus, vel adhuc cerneret caduca scele-
 " ta magnanimorum heroum, aliquumdiu erecta in campo
 " conflictus, ubi aplustrium instar, jocularios experiuntur lusus
 " volitantis zephyri."

23. "ITA sese in plantis habere, (says
 " M. *Wahlbom*) dictitat primo oculus. Flo-
 " re florescente, et polline antherarum vo-
 " litante, quod stigmati pollen inhaereat, pri-
 " ma fronte obvium est (u)." But it is not at
 all obvious, far less does the eye see, that ge-
 neration is thus accomplished ; for this dust
 as often bespatters the *petala*, &c. Neither
 does what he adds concerning the florescentia
violae tricoloris, *gratiolae*, *iridis*, *campanulae*
et syngenesiarum, in the least favour him.
 For, tho' all he says were true, these make
 but a very inconsiderable part of the vege-
 table kingdom. Yet they seem rather to
 make against him. For in these compound
 flowers, which he calls *syngenesiae*, the
 " *antherae* sunt lineares, erectae, lateribus
 " coalitae in cylindrum tubulatum. *Stylus*
 " filiformis erectus staminum longitudine,
 " antherarum cylindrum perforans. *Stigma*
 " bipartitum, laciniis revolutis, patentibus."
Lin. Gen. pl. p. 370. To which if we add,
 that these *antherae* split, and emit their dust,
 on the outside, not inside, of this *cylinder*,
 while the *stigma* is commonly pretty far a-
 bove

(u) *Amaen Acad.* 1. p. 90.

bove it; what can reasonably be inferred from this structure, but that Nature designed this dust should be thrown away as useless, if not hurtful to the *stylus*?

AGAIN, the *stylus* of the *campanula* is commonly much longer than the *stamina*, and bristly a little above them, as it were to hinder their access to the *stigma*. The *stamina* of the *iris* are hid under the outside of the segments of the *stigma*, the depressed *petala* being frequently bearded or hairy under them. So no plants could be here more improperly alledged; for, by the structure of their flowers, it is evident, that the *pollinis il-lapsus supra stigmata nuda* is impossible: nor can Mr. *Wahlbom* deny it; "*campanula*, (says "he) a caeteris in eo differt, quod pulvis lateri hispidi styli adfigatur, et exinde per "certos canales stigmati comunicetur. *Iris* "particularem nobis ostendit structuram; "stigmata enim sese dilatantia, antheras omnino operiunt; illum tamen ad petala referunt situm, ut, aura subeunte stigmata, "pollen per rimas illorum ascendat." What eye ever saw these *canales*, or *rimae*, with the *pollen* rising in them? how, or why rise to the *stigma* thus situated?

As

As for his observations on the *viola tricolor*, I pass them, because nothing to the purpose; and also too smutty for *British* ears. But let us hear what the great *Linnaeus* by himself says:

24. "GENERATIONEM vegetabilium fieri, &c. (says he) confirmat oculus. Pollinem intrare germina credidit *Morilandus*; ejusdem essentiam extrahi, mediante stigmate madido, statuit *Vaillant*; pollinem aceris rumpi in humore vidit *Bern. Jussiaeus*; omnem pollinem in humore explodere auram seminalem, confirmat *Needham* (x)." But *Morland's* opinion is fully confuted by *Vaillant*, who maintains only that the vapour, or volatile spirit of the male dust, enters the *trackiae* of the *stylus*; but not a word can I find in his *discourse*, concerning the extraction of the essence of the dust by means of the moist stigma: and, altho' it should all burst in moisture, I do not see how it thence follows, that it thus sends out an *aura seminalis*; especially, since it does not appear, that the *stigmata* must necessarily be moist in order to fertility: and it is observed by Mr. *Wahlbom*

(x) Phil. Bot. p. 91.

Wablom (y), That, "In omnibus fere floribus conspicitur, quomodo, aëre humido, flosculos complicant, ne aqua pollen attingat." By no means, therefore, can the eye be said to confirm the sexual scheme.

25. THE *second* argument for this scheme is taken from the proportion which the *antherae* bear to the *stylus*.

"Ex proportione quoque, (says *Gesnerus*) (z), verosimiliter judicamus, cum, pro magnitudine et numero seminum, ipsa quoque stamina majora sint vel numerosiora." But that this is evidently a mistake, will appear to any one that will take the pains, to compare *Linnaei monandriae* with his *polyandriae*; or the *cannacorus* with the *prunus*, and other stoned fruits: the *syngenesiae* have five *stamina* for one seed, and that, not seldom, a very small one: the *umbelliferae*, as many for two seeds, &c.

"Ita sese in plantis habere dicuntur secundo proportio: plerumque stamina et pistilla eandem ferunt altitudinem, ut eo melius ad stigma pollen, mediante vento, accedat; in

(y) *Amaen Acad.* 1. p. 93.

(z) *Dissert.* p. 91.

“ in quibusdam vero non, ubi singularis ob-
 “ fervatur processus foecundationis (a).” But
plerumque dictitat nihil, especially since there
 are a great many *genera*, where no such pro-
 portion or singular process takes place, as in
 almost all the *flosculosi*, *semiflosculosi*, *radiati*,
liliacei, *caryophyllæi*, &c. and of the fix
 plants instanced as singular, in three, *viz.*
dianthus, *nigella*, and *passiflora*; the *pistilla*, any
 curvature notwithstanding, continue high
 above the *stamina* (b).

This argument is thus explained by *Lin-*
næus. “ *Proportio: stigmata sese flectere ad*
“ antheras, dein exseri ex diantho, passiflora,
“ nigella, patet. Pistillum ubi brevissimum,
“ connivent antherae supra stigmata: saxi-
“ fraga, parnassia. Connivent dum efflant pol-
“ linem antherae in celosia. Comprimit corolla
“ digitis antheras ad stigmata in Teucrio (c).”
 Here not a word of proportion confirming
 his scheme; no instance of it; but only rea-
 sons for inequality. No matter, therefore,
 whether it be so in these or not.

26. THE

(a) WAHLBOM, *Amaen. acad.* 1. p. 90.

(b) See Pontedera for more instances. *Anthol.* 1. 2. c. 8.

(c) *Phil. Bot.* p. 91.)

26. The *third* arg^t is, "Ex loco staminum
 " et pistilli, non leve argumentum petere pos-
 " sumus; nam in plerisque plantis, floribus
 " hermaphroditicis peditis, stamina ambiunt
 " ovarium, et ea ratione ut maxima pars ge-
 " niturae ad stigma accedat." Gesnerus (c).
 Can the *stamina* surround the *ovarium*, in the
monandriae, *diandriae*, &c? but this needs no
 answer; neither what he adds concerning the
pistilli fabrica, and *antherarum materies*, which
 is nothing to the purpose.

"Tertio, locus. Etenim stamina plerumque
 " pistillum ambiunt, ut ventorum ope semper
 " quidquam pulveris attingat stigma." Wabl-
 bom (d). But the learned author cannot but
 know, that the *stamina* standing round the
pistillum can never prove the necessity of the
 dust's falling on the *stigma* of every plant, in
 order to its foecundity; especially when this
 is not always their situation. He adds, "Mo-
 " neciae flores masculi plerumque supra flo-
 " res foemineos collocantur, ut pollen eo me-
 " lius in pistillum decidat;" and instances
ricinus among others: but, sure I am, the
ricinus vulgaris B. p. has all its female flowers

H h

above

(c) Dissert. p. 9.

(d) Amaen. Acad. 1. p. 91.

above the male flowers ; the *stigma* of the uppermost being commonly some inches above the nearest *stamina*.

“*Locus*. Nunquam pistilliferae sponte nascuntur sine staminiferis in eadem terra; prodeunt ex eodem semine ambae.” *Linnaeus* (e). But this cannot be proven. Yea, *Camerarius* asserts the contrary (f). But, granting it true, it proves nothing; confirms nothing,

27. Argument 4th, “*Ex tempore quoque* vegetationis harum partium concludere datum est: namque in antheris, *stamina* farinam foecundantem eo tempore continent, quo pistillum viget, deinde, excusso pulvere peracta foecundatione, perit stamen; succus copiosior in pistillum devolutus, efficit ut fructum maturescat,” *Gesnerus* (g). But the *stamina spinaciae, mercurialis, cannabae, mays, juniperi, violae martiae, &c.* shed their dust commonly before their *stigmata* are visible.

“*Quarto tempus*. PRIMUM hic attendendum venit quod *stamina* et pistilla una proveniant,

(e) Phil. Bot. p. 91.

(f) See No 10. supra.

(g) Dissert. p. 91.

“provenient, exceptis tantum paucissimis,
 “Alterum, quod ubicunque flores masculi
 “distinctis a foemininis gaudent thalamis, aut
 “in eadem aut diversa planta, et ubi masculi
 “flores nec perpendiculariter supra foemineos
 “erecti sunt; ibi, florescentia ante foliorum
 “exortum peragatur, necesse est; ne, foliis
 “intervenientibus, inhibeat foecundatio:
 “ex. gr. in moro, visco, mercuriali perenni, &c.”
 Wahlbom (b). How justly these three are in-
 stanced, any body may judge. But since he
 admits of exceptions, as to the first *notandum*;
 and many other plants emit the flowers be-
 fore the leaves; this argument proves no-
 thing.

“TEMPUS. In declinis flores ante germi-
 “nationem foliorum *plerumque* prodeunt, ne
 “folia tegant pistilla; *salix, populus, cory-*
 “*lus, &c.*” Linnaeus (i). Here, as com-
 monly, we find *plerumque*; and an imagina-
 ry reason.

28. Argument 5th, viz. *pluviae*; “Fit in-
 “de ut, dum *pluvia* eluit pulverem staminum,
 “germina pistilli decidant, aut in fructus
 “nascantur abortivos, ut stillant vites, perco-
 “quantur,

(b) Amaen. Acad. 1. p. 92.

(i) Phil. Bot. p. 91.

“ quantur, marasmo exarescant, locum con-
 “ cedant insectorum nidis et eorum evoluti-
 “ oni, ut fruges ustilagine pereant, et quae
 “ sunt alia vitia. Sedulo itaque Natura ubique
 “ sollicita fuit, ut genitura illibata ad pistillum
 “ perveniret,” *Gesnerus* (*k*). But how does it
 appear that such are the consequences, of the
 dust's being washed away by rain? Has too
 much moisture no bad effects, after the *stami-*
na have shed their dusts? Does it not fre-
 quently rot the plants? &c.

“ Quinto *pluviae*. In omnibus fere flori-
 “ bus conspicitur, quomodo, urente sole, sese
 “ expandant, vespertino vero tempore, et
 “ aëre humido, flosculos complicant, ne
 “ aqua pollen antherarum attingat et coagu-
 “ let, quo facto, ad stigmata efflari nequeat;
 “ at stigmate, mirum sane! semel foecunda-
 “ to, nec vespere, nec pluvia ingruente,
 “ sese contrahant flores,” *Wahlbom* (*l*). But
 our author knows, that many plants close
 their leaves in the night or in rain; as
 the *acaciae*, *mimosae*, &c. that many open
 their flowers in the night, and shut them
 when

(*k*) Dissert. p. 91.

(*l*) *Amaen Acad.* 1. p. 93.

when the sun is hot; as some *cerei*, *ketmie*,
xyla, *lychnis noctiflora*, *mirabilis peruvia-*
na, &c. (m) Does not the *passion-flower* keep
open in the night as well as day, until it shut
up for good and all, and that whither it be
sun-shine or rain? He adds, "*Secale florens*
"*antheras filamentis insidentes exferit, quo*
"*tempore, si pluvia cadit, pollen congloba-*
"*tur, hincque annonam difficilem auguratur*
"*agricola, nec immerito; grana enim im-*
"*minuuntur exinde, quod plerique flosculi*
"*abortum passi sint.*" That *secale*, *triticum*,
many *gramina*, *plantagines*, *pimpinellae*, &c.
thrust out the *apices* on pretty long *stamina*,
when in flower, I deny not; but that rain at
that time causes scarcity of any of them, I
never observed: and altho' it were granted,
that this commonly happens, How does it
appear that rain causes the *flosculi* to abort,
or this abortion causes scarcity of *secale*?
since, at the same time, the other plants are
sufficiently fertile, yea the manner of flower-
ing in some of these plants, seems to afford an
argument, not contemptible, against the sex-
ual

(m) "*Mirabilis est planta quae tam speciosos flores nocti*
"*atrae objicit, et sereno diei subtrahit.*" Lin. H. Cliff.
p. 54.

ual scheme. For in *secale* and *triticum* too, there are three *stamina* for one *germen*, both included within, and well covered by the same husks (be they a *corolla* or *calyx*, or both, no matter here) before the flowering time. Now if the dust be necessary to the impregnation of the seed, and, in order to this, it must fall on the *stigma*; Why is not this accomplished, before the covers open? Why are the *stamina* so long? Why do they thrust out the *apices* so far from their *stigmata*, and throw out, if not all, at least the greatest part of their dust, in the common air? Does not all this look as if this dust, was not designed by Nature for the impregnation of the seed? as is often noticed.

“*Pluviae* (says *Linnaeus*) (*n*) combibunt
 “pollen, ut in *stigmata* cadere nequeat;
 “hortulanis notissimum in drupiferis, (so
 “he calls the *amygdalus*, *persica*, *prunus*,
 “*armeniaca*, *cerasus*, *laurocerasus*, &c.) et
 “pomiferis. Agricolis detestabilis in agris
 “secalinis, (why not also *triticeis*?) *Fu-*
 “*mus* idem etiam facit, absorbendo humi-
 “dum *stigmatis*.” So the *stigma* must nei-
 “ther be wetted nor dried. But this is fully
 answered

answered above. *Non causa pro causa* is here very frequent.

29. THE *sixth* argument, which the Sexualists triumph in, as a demonstration of their doctrine, is taken from the culture of the *palm-tree*. “*Instar omnium argumentorum esse potest* (says J. Gefnerus) (o) *modus quo foecundatio palmae dactyliferae, ad obtinendos dactylorum fructus maturos, apud Persas instituitur, a cl. Kaempfero, in his locis, annotatus.*” It would be too long to transcribe all that modern authors have said concerning this tree. The reader may, if he pleases, consult John Leo (p), Prosper Alpinus (q), Hadgi Mustapha Aga (r), Engelbertus Kaempfer (s), Pere Labat (t), Christ. Got. Ludwig. (u), &c.

30. THESE authors are agreed only in this, that the date-bearing *palm-tree* has no flowers: and unless the flowering or male *palm-tree* be sufficiently near, or the dust of its *apices* be some

(o) Dissert. p. 85.

(p) Harris collect. vol. 1. p. 347.

(q) De pl. Aeg. p. 24.

(r) T. Just. p. 69.

(s) Amen. exot. p. 706.

(t) Voyage aux isles de L’Amerique, Hague edit. vol. 1. part 2. p. 209.

(u) In J. Gefner. diss. p. 86.

some how conveyed into the *spatha* of the female; its date stones will not grow. I shall pass the very wide differences among them, as to the manner of conveying it; and only observe, that without the influence of the male, the female will either be barren, or cast her unripe fruit, according to the anti-ents, and *Alpinus*; that the dates will want stones, be harsh, and not eatable, except by camels and cattle, *Hadgi*. "Omnia sua fructuum rudimenta, indeclinabili abortu dimittunt, *Kaempfer*. "In fructu pulpae loco adest cortex durior, ficcus, adstringens, officulum vel nullum vel tenue," *Ludwig*. which are all contradicted by *Labat*.

31. "It is pretended, says that reverend father, that the *date-tree* is male and female; that the male bears blossoms, but no fruit, that being left to the care of the female; but that she would carry none, if she had not the male by her, or at least within sight of her." I am sorry that I cannot subscribe to this opinion of the naturalists, but it is a most certain experiment, directly opposite to their sentiments, that hinders me; for we have a *date-tree* beside our monastery in *Martinico*, which carries fruit, tho' single: whether it is male
or

or female, I know not; but this I know for certain, that there was not another of the kind within two leagues of it. Whence we may conclude, that the presence of the male is not necessary to render this tree fruitful, as naturalists pretend." He says indeed also, that the stones of the dates of the islands will not grow; so that those who would raise *palm-trees*, are obliged to plant the *Barbary* dates; and that dates do not ripen so perfectly in *Martinico* and *Guadaloupe*, as they do in *Africa*, in *Asia*, or even in *St. Domingo*." For altho' our dates, says he, become soft, yellow, and as it were luscious, and in a word, appear perfectly ripe; yet they still retain a certain sharpness, which shews they want at least some degree of maturity." Thus *Pere Labat*. l. c.

Jo. Bauhinus (u) relates, that he saw only one date-bearing *palm-tree* at *Montpelier*; "Centesium annum superare creditur, (says "he) et vulgo ibi persuasum, ante grandem "aetatem fructum non proferre, et vix ante "quingagesimum annum, ut quidam per- "hibebant." Hence *Pontanus's* fiction is easily accounted for, if there be any truth in it;

I i

and

(u) Hist. i. p. 360.

and perhaps some others, particularly concerning the amours of these trees; for which see *Pliny* (x), and *Cassianus Bassus* (y).

39. BUT I cannot omit the opinion of *Herodotus*, the most ancient author, who has left any thing concerning the usefulness of the male, to the female *palm-tree*; “ In *Babylonia* agris, says he, *palmae* magna ex parte fructiferae proveniunt; ex quibus non solum cibum, vinum, et mel conficiunt, sed etiam eodem modo quo *ficus* curantur. *Palmarum* enim, quas *Graeci* masculas vocant, fructus *palmis* glandiferis alligant, ut earum fructum maturet *culcx* subiens, ne ex arbore is defluat. Ferunt enim *palmarum* mater (z) culices in fructu, quemadmodum caprifici.” Thus *Herodotus*, as rendered by *Bod. a Stapel* (a). Who elsewhere (b) quotes a famous traveller for such a culture of this tree, as confirms *Herodotus*’s opinion (c).

AGAIN

(x) L. 13. c. 4.

(y) In *Theoph.* p. 103.

(z) ψννα.

(a) In *THEOPH.* p. 115.

(b) P. 103.

(c) “ *Agriculturae etiamnum, ut refert doctissimus GUIL- LAND, in Arabia, Aegypto, Mesopotamia, Iudaea, Phoenicia, et tota Syria, volentes cavere ne foeminae, aut sterilitatis nox-* “ am

AGAIN *Joan. Veslingius*, who was long in *Egypt*, differs not a little from *Alpinus*, as to the culture of *palm-trees* there: "Causa tam
" foecundi proventus (says he) in telluris ha-
" bitudinem referenda est, arenosam scilicet
" et salsam, plantae huic gratissimam. Vidi
" his locis in palmarum veluti sylvis, terram
" copiosissimi nitri calida nive late consper-
" sam, vel aquis Nili, vel roscida noctium
" humiditate fervidiore sole perustis. Nec
" opus hic *maritali cinere* palmae effoeminatae
" vigorem incitare. Flantibus enim ab au-
" stro per *Æthiopiam*, et steriles Arabiae de-
" fertae campos urentibus, ventis; ingens ni-
" trofi pulveris sublata vis, abunde cacumi-

na

" am incurrant, aut fructum, quem aliquando imaginatione
" libidinis, ut ita dicam, concipere solent, ante maturitatem
" amittant, ita ordinant utriusque sexus palmas, ut mares eo
" saltem intervallo a foeminis distent, quo pulvis, ventorum
" flatibus a foliis masularum sublatus, in foeminarum folia
" incidat; idque satis ad foecunditatem, et fructus matura-
" tionem facere compertum est. Sed mirum dictu! quod si
" qua procul a mare abstiterit, ut neque pulvis, neque aura,
" odorve ejus, ad eam permeare possit, excogitaverunt colo-
" ni *funem a mare religatam ad foeminam usque producere*; at-
" que, ita quasi maritali vinculo copulatam, masculi virtute
" clanculum per funem irrepente, foecundam fieri, quae prius
" in ea sterilecebat solitudine." It is easy to conceive how
insects may creep along a rope, but not how a powder or
dust can be thus conveyed from one tree to another.

“na palmarum vegetat.—Meminique sic o-
 “nustum fuisse dactylis suis unicum race-
 “mum, ut eum attollere a terra prae pondere
 “vix sustinerem (*d*).” And *Alpinus* himself
 is obliged to own, that the fruitfulness of the
palm-trees in the deserts of *Arabia*, is not ow-
 ing to any artificial culture, but to the winds
 carrying the dust and flowers of the male to
 the female (*e*). Is it probable, that nature
 has left the fertilising of so useful a tree, to
 the uncertain motions of the air?

BESIDES, that great and curious botanist
Tournefort, after mentioning the opinion of
Theophrastus, *Alpinus*, and others, concerning
 the male *palm-trees*, adds, “Cum in Hispania
 “Baetica, palmarum feraci, a prudentioribus
 “viris de hac re sciscitarer, certum nihil ac-
 “cipere potui. De *lupulo* certius loquor. In
 “Horto Regio Parisiensi, luxuriat fructibus
 “quotannis onustus. Qui vero floribus gau-
 “det, non occurrit nisi in insulis Sequanae et
 “Matronae, longe distantibus: in Horto Re-
 “gio tamen semina profert” (*f*).

THIS

(*d*) Velling in *Alpin*. c. 7. p. 11.

(*e*) V. *Alpin*. de plant. *Æg*. p. 25.

(*f*) *Inst*. p. 69.

THIS objection against the sexes of plants drawn from the *lupulus*, appears to be strengthened by the answer made to it; which is this, “ *Humulus duplex omnino est*; “ *unus floribus superbit staminiferis, alter pistilliferis*; idque quod *fructum* vulgo vocant, “ *est calyx tantum explicatus et elongatus*: “ *hinc humulus*, quamvis foemina, nec foecundata, conos tamen proferre valet. Hoc “ Tournefortium decepit, ne sexum plantarum “ agnosceret, quum *lupulus* (foemina) in hortu Parisiensi luxuriabat, fructibus quotannis onustus; qui vero floribus gaudebat (mas) “ non occurrebat nisi in insulis Matronae et Sequanae multum distantibus (g). Idem “ fit in *moro* et *blito*, cujus baccae calyces sunt “ succulenti; minime pericarpia, seu ovaria ” (b).

34. FOR I am at a loss to find wherein Tournefort was deceived. He gives an accurate description, as well as elegant figures, of the parts of the flower and fruit, as standing on different plants (i), without which the character of the *humulus*, in *Linnaei Genera plantarum*

(g) Tournefort, Hag. p. 69.

(b) Amaen. acad. 1. p. 99.

(i) Vid. T. p. 535. t. 309.

plantarum (k) is not very intelligible to a beginner. But he calls the enlarged *calyx*, *fructum*: the cones he calls *fructus*; and so they are, in the most proper sense of the word. *Linnaeus* himself, in his *Fundamenta bot.* teaches, that “*essentia fructus in semine consistit*”(l); and, in his *Philosophia Botanica* (m), “*fructus ex semine sive pericarpio, sive non tectum sit, dignoscitur.*” No matter therefore, whether ye call these cones *calyces elongatos*, or *fructus*, if they contain seeds: and *Tournefort* expressly adds, “*In horto regio semen profert*,” which Mr. *Wahlbom* is pleased to omit, for what reason I shall not say. As for the *morus* and *blitum*, I see not why their *baccae succulentae* may not be called *fructus* also; especially since *Linnaeus* (n) gives *blitum* a *pericarpium*; and describes a *pericarpium* to be “*viscus gravidum feminibus, quae matura dimittit*”(o): and consequently *Wahlbom* and he don’t well agree. But, to return to the *palm-tree*,

35. SEXTO,

(k) P. 477.

(l) § 88.

(m) P. 56.

(n) Gen. pl. p. 5.

(o) Phil. Bot, p. 53.

35. "SEXTO, *Palmicolas* palmarum spadices masculos divellere, eosdemque supra foeminas collocare, memoriae mandarunt, *Theophrastus*, *Plinius*, *Alpinus*, *Tournefortius*, *Kaempferus*, alii: quo neglecto datyli acerbi, et nucibus destitutae, fiunt," *Wahlbom* (p). This is answered above (N° 31): then the author gives a long paragraph out of *Kaempfer* (q); as does *Gesnerus* (r), from the same page; yet they differ widely. I have not at present *Kaempfer* by me; but may be allowed to notice one thing, not very credible in each, according to their principles, viz. "Nemus foecundari universum potest ab una phoenice florida," *Gesner*. "Singulare quod spadices exsiccati ad thalamos apti sunt, et in annum posterum, salva virtute, asservari possunt," *Wahlbom*.

36. "PALMICOLÆ: notissima *Theophrasto*, *Plinio*, *Kaempfero*, aliisque. *Pistaciae* cultura in Archipelago: *Tournefortius*. *Caprificatio* veterum, et adhuc in Archipelago, per insecta." *Vid. dissert. nostr. de ficu*,
Linnaeus

(p) *Amaen*, Acad. 1. p. 94.

(q) *Amaen*. p. 706.

(r) *Dissert.* p. 85.

Linnaeus (t). Neither *Theophrastus*, *Pliny*, nor *Kaempfer*, relate the culture of *palm-trees*, as from their proper knowledge; and are contradicted by an eye witness, *Pere Labat*.

As for the culture of *pistacia* in the *Archipelago*, for which *Tournefort* is alledged, I cannot find any such thing in his works. Such a culture, indeed, is mentioned by the editor of *M. Geoffroy's Materia Medica* (u), as practised in *Sicily*: but he neither says he was in that island himself, nor tells us who informed him. However, if such is the culture of the *pistacia*, either in *Sicily* or the *Archipelago*, it seems to be very modern, and founded on the imaginary impregnating virtue of the dust of the *apices*; and, no doubt, the *terebinthus* will soon be treated the same way, it can do no harm; tho' it is not very probable, that *deficiente tali impregnatione fructus abortiant* in this tree, more than in the *palm-tree*.

37. CONCERNING *caprifigation*, if you consult *Theophrastus* (x), *Pliny* (y), and *Tournefort*

(t) *Phil. bot.* p. 92.

(u) *Tom. 2.* p. 417.

(x) *De caus. pl.* l. 2. c. 12.

(y) l. 15. c. 19.

fort (z), or *Pontedera* (a) only, who quotes all the three, together with the learned Dissertation our author refers to, which I take to be *Cornelii Hegardt Historia naturalis et medica ficus* (b); it will be easy to judge, whether it is at all probable, that the fig insects carry the dust of the *caprificus* to the unripe figs, and thus impregnate the seeds (c).

K k

38. IF

(z) Voy. let. 8.

(a) Anthol. l. 2. c. 33, 34, 35.

(b) In C. LINNÆI, *Amaen. Acad.* vol. 1. p. 213. 243.

(c) "Capido ficus nobis dicitur, quem antiqui *psenem* seu
" *insectum* vocarunt *ficarium*, et PONTEDERA, Anthol. 172.
" descripsit, estque species ichneumonis. Hisce ichneumo-
" nibus jam mutatis, aliisque instructis, tempus adest, quo
" caprificus, seu ficus mas, florescit, h. e. farinam edit anthe-
" rarum; tunc ichneumones e caprifici cavitatibus, farina,
" *molitoris* instar e mola sua prodeuntis, obducti evolant, et
" conjugibus acquisitis de ovis pariendis solliciti sunt: hinc
" ad singulos grossos transvolantes, cavitates ficus foeminae,
" dolii instar, clavis ferreis vel spiculis seu pistillis ab omnibus
" lateribus intus completas, intrando, non possunt non fari-
" nam illam, qua contacti sunt, excutere. Patet igitur hoc
" modo, *ficum* hanc foeminam facillime impregnari." Thus
Mr. HEGARDT, *Amaen. Acad.* 1. p. 231. A fine appen-
dix for PONTANUS's poem! But,

ACCORDING TO PONTEDERA, "Ficarii culices, forma
" ad vespas, nascendi autem modo ad muscas, accedere mihi
" videntur. Ut primum grossi grandiusculi facti apertum fun-
" dum ostendunt, ingrediuntur foemellae, et ex infimo veni-
" tre producta tuba, foeturae in frumentis nidum excavant, et
" ovula

38. If it be still alledged, that the infocundity of the date stones of *Martinico* demonstrates the foecundating virtue of the male dust ; I answer, by no means : for *Pere Labat*

“ ovula deponunt. Nascuntur in his vermiculi, qui deinde
 “ nymphae evadunt immobiles, durae, corpore oblongo, ca-
 “ pite cum dorso luteolo, caetera primum albae mox nigrae.
 “ Perforato deinde nido, exit animalculum, plerumque non-
 “ dum pinnis explicatis. Egressum statim senectam deponit
 “ a capite incipiens. Tunc flavum cernitur, quod tamen
 “ mox exsiccatum nigrum evadit. Dum vero involucris
 “ spoliatur, huc et illuc revolvitur ; et propterea apicum
 “ pulvisculo, quo tota pomorum cavitas repleta est, infarci-
 “ tur, quippe molliusculum. Quare e grossis egressum, et
 “ sole exsiccatum, pulverem discutit ad hunc modum :
 “ stans quatuor anterioribus pedibus innititur, et duobus po-
 “ strems abdomen, lumbos, pinnae pulvere mundat, iterum
 “ atque iterum cruribus detergens ; deinde quatuor posteri-
 “ oribus sese librans, duobus anterioribus caput, dorsum et
 “ cornua purgat. Quemadmodum feles et alia elegantiora
 “ animalia solent. Tunc, deposito onere, evolat.” An-
 “ thol. p. 174. And p. 175, he adds, “ Hujusmodi animalcu-
 “ lis tota grossorum caro et frumenta corrumpuntur. In sativae
 “ vero ficus pomis, haec animalcula nunquam inveni : num
 “ vero ad haec volent ignoro ; sedulo hoc mihi inquirenti,
 “ nullum sane in sativis ficubus apparuit. Neque enim Ita-
 “ lia caprificationis indiget, sed sine grossis sativae ficus sua
 “ coquunt poma. In Graecia hoc culturae opus perpetuum
 “ non est, serotina poma non caprificantur ; neque praeco-
 “ ciorum in macro solo, et in aquilonio, in ipsa Graecia
 “ ulla fit caprificatio. Pomum caprificatum bonitate inferius
 “ est non caprificato et insuavius. Hinc illi qui in Graecia
 “ ficus

bat does not say that he tried them; but in general, that the date stones of the *French* islands would not grow; and, it can scarce be supposed there were no males among all their *palm-trees*. Besides many things concerning *palm-trees*, which appeared to be as well attested, are found to be vulgar errors. But, granting that they really are barren; this may be owing to the climate, to the soil, to bad culture, or to the want of *Pontedera's culices*, rather than of the male dust, for any thing yet appears.

39. ARGU-

"*ficus venundabant, quo facilius emptores allicerent, ovum & replera iterum atque iterum clamitare solebant.*" And, after explaining the use of caprification, he adds, "Quare concludendum, caprificationem in Graecia ob externas causas esse necessariam, nequaquam ob ficus naturam, cum alibi poma coquant non caprificata. Eadem etiam de causa *palmas* in quibusdam regionibus esse caprificandas, in aliis minime; id autem per *culices* fieri, non vero per affectionem, quam apices embryonibus communicent, satis demonstratum est." See Anthol. l. 2. c. 34, and 35. p. 172, &c. Now since M. HEGARDT's *Cupid*, or *Miller* does not goe abroad, till well brushed, and freed of the dust; since there is no caprification in *Italy*; and since, this notwithstanding, he, and LINNAEUS also affirm, "*Ficus* in *Hollandia* quotannis prolici e seminibus, vel fructu lacerato terrae commendato, fructu tamen illo ex *Italia* alato." Amaen. Acad. 1. p. 233. Exam. Epicris. p. 16. &c; I leave it to the reader to determine, whether caprification affords an argument for, or against the sexes of plants.

39. ARGUMENT 7. "Septimo flores nu-
 "tantes. Cum pollen masculus, plerumque
 "aëre specificè gravior, difficile sursum ten-
 "deret; apud plerasque plantas, pistillum lon-
 "gius gerentes, florem nutantem fecit Crea-
 "tor, ut stigma eo melius attingat pulvis:
 "e. gr. in *galantho*, *leucoio*, *cyclamine*, *narcis-
 "so*, *fritillaria*, *campanula*, *erythronio*, &c." *Wahlbom* (e). "Flores nutantes gaudent
 "pistillo, staminibus longiore, ut cadat pol-
 "len in stigma: *campanula*, *leucoium*, *galan-
 "thus*, *fritillaria*," *Linnaeus* (f).

I readily grant, that the *pollen masculum*,
 or *pulvis apicum*, is heavier than air, it being
 often heavier than water; and also, that some
flores nutantes have the *pistillum* longer than
 the *stamina*; but cannot allow the reason to
 be "ut cadat pollen in stigma:" for thus it
 must fall on the back of the *stigma*, when
 the *stylus*, is there thickest; or fall by it quite,
 when there slenderest; and, in neither case,
 can thus have ready access to the seed. Again,
 in many genera of plants, not only in different
species, but even on the same stem, some
 flowers hang down, others stand erect, o-
 thers

(e) *Amaen. acad.* 1. p. 95.

(f) *Phil. Bot.* p. 92.

thers horizontal, while the *pistillum* and *stamina* bear the same proportion to one another : e.g. some species of the *narcissus*, *campanula*, *jilium*, &c. And altho' every *stos nutans* had the *pistillum* longer than the *stamina*, yet it could not thence be inferred with any probability, that the design of the great Creator, in thus forming them, was what our authors allege, unless every erect flower had its *pistillum* shorter than the *stamina*, which is far from being fact, as is proven above (g).

40. ARGUMENT 8. " Videte et admire-
 " mini solertiam, quam Natura adhibet in
 " plantarum aquaticarum, quae farinam foe-
 " cundantem habent, floribus. Tempore
 " florescentiae, flores specificè leviores redditi,
 " ultra aquae superficiem attolluntur, ut in
 " aëre foecundatio fiat, nec humiditate dilua-
 " tur genitura : dum vero sub aqua flores ad-
 " huc reconduntur submersi, follicite per pe-
 " talorum commissuras clauduntur, apicibus
 " versus stigma inclinatis, et versus interio-
 " ra tantum farinaceis, exteriori superficie
 " membranacea et lata ; ut in *nymphaea* et
 " affinibus apparet," Gesnerus (h).

OCTAVO

(g) Vid. No 25.

(h) Dissert. p. 92,

“OCTAVO *submersi*. Plantae haud paucae
 “caule sub aqua latent; instante vero flores-
 “centia, enatant flores, ut *nymphaea*, &c.
 “Aliae vero sub aqua omnibus suis partibus
 “occultantur; ut *myriophyllum*, *stratiotes*, *po-*
 “*tamogetones* plerique, qui omnes, sub flores-
 “centia, spicam floris supra aquas exserunt,
 “deinde iterum, perfecta florescentia, de-
 “mergitur spica,” *Wahlbom* (i).

“*Flores submersi* adscendunt sub florescen-
 tia: *nymphaea*, *stratiotes*, *myriophyllum*, *potamo-*
 “*geton*, *hydrocharis*, *valisneria*,” *Linnaeus* (k).

ALTHOUGH some aquatic plants blow on-
 ly above the water, it cannot be proven that
 all do so: yea it is certain, that many subma-
 rine ones fructify under water. But, grant-
 ing all the alledged facts, it by no means fol-
 lows, that the design of Nature is, to procure
 the better access of the dust to the *stigma*; but
 rather that it may be dispersed in the air. For
 since, under water, the flowers “solicite per
 “petalorum commissuras clauduntur, apici-
 “bus versus stigma inclinatis, et versus interi-
 “ora tantum *farinaceis*,” the dust, or its *aura*
seminalis, must there have much easier access

to

(i) *Amaen. acad.* 1. p. 96.

(k) *Phil. Bot.* p. 92.

to the *stigma*, than it can have, when the *stamina* are separated, and exposed to the winds; especially, if it be true, as Mr. *Vaillant* has it, that, in hermaphrodite flowers, and such, according to the Sexualists, is the *nymphaea*, and some other aquatics, the dust is not thrown out at once with such violence, as it is where the sexes are separated, “ sed actum
“ generationis (adds Mr. *Gesner*) floribus clau-
“ sis exercent, dum stigma adhuc intra me-
“ dias antheras continetur.” (l)

41. I mention these two learned authors, tho' I have the misfortune to differ from them, not only because they are commended by *Linnaeus*, especially *Vaillant*, of whom he says, “ primus clare sexum exposuit (m),” but also that I may help them to a better instance, than the *parietaria*, of the sudden explosion of the dust of the *apices* in barren flowers: it is the common flowering nettle, or *urtica urens maxima*, B. p. 232. (n); for one cannot observe

(l) Vid. n. 21. supra.

(m) Bib. bot. p. 173.

(n) That is for a specimen of botanical superfluity; *urtica foliis cordatis amentis, cylindraceis, sexu distinctis, mas*, Fl. lap. p. 299; *urtica foliis oblongo-cordatis, dioica*, H. Cliff. p. 440; *urtica dioica foliis oblongo-cordatis*, Fl. suec. p. 282; *urtica mascula*, Syst. Nat. 133; *urtica perennis*, Amaen. Acad. 2. 25. 99.

observe this stinging *nettle* for a few minutes, in almost any of the summer months, if the sun shines, especially before noon, but he must see many little clouds of dust, thrown with a sort of explosion out of the bursting *apices*, which soon disperse and fall down. This, however, being a single instance, and by me not observed in *spinacia*, *mercurialis*, *cannabis*, or any such barren plant; no general conclusion can be drawn from it.

42. ARG. 9. "Nono, *Syngenesia frustranea*.
 "Flores compositi variis modis fabricati sunt.
 "— *Polygamia frustranea* foeminis exsultat
 "maritatis, totum discum occupantibus; flos-
 "culi vero foeminei radium constituentes, ob
 "defectum stigmatis, abundante licet disci
 "pulvere, familiam propagare nequeunt,"
Wahlbom (o). I omit the rest, since *Linnaeus*
 (p) says only, "*syngenesia frustranea*: ubi
 "stigma deest, ibi nulla foecundatio: in ra-
 "dio *centaureae*, *helianthi*, *rudbeckiae*, *coreo-*
 "*psidis*." But, in the *radius* or *corona* of all
 these flowers, the *stylus* is also wanting, as
 well as the *stigma*; which may be many
 ways

(o) *Amaen. Acad.* 1. p. 96.

(p) *Phil. Bot.* p. 92.

ways necessary to the ripening the seed. It does not therefore appear, that this proves or confirms the foecundating virtue of the dust.

43. ARGUMENT 10. is a very extensive one. "Ita sese in plantis habere dicitur de-
" cimo *omnium florum genuina consideratio.*
" Brevitatis causa nonnullos tantum hic exa-
" minare lubet," *Wahlbom* (z). And, for
brevity's sake also, I shall here pass them all,
some of them having been noticed already,
as others will be below; with this obvious
note, that such a general position can be pro-
ven only by an as general induction; which
is altogether impracticable.

44. BUT since *Malpighius*, whom I reckon
one of the most genuine contemplators of
flowers, observes (a), that, "Turgentibus
" orbicularibus corporibus quibus staminum
" capitula replentur, *exsiccataque continente*
" *capsula*, foras prodeunt globuli minimi et
" disperguntur;" and that this exsiccation
does not, cannot well happen, before, by
opening of their covers, the *stamina* be ex-
posed to the free air; and since in fact, the

L 1

apices

(z) *Amaen*, Acad. 1. p. 97.

(a) P. 63. edit. in 4to.

apices do not split, at least generally speaking, so far as I have observed, until their covers open; it appears to be a more natural inference, that this dust is not designed to fall on the *stigma*, or to impregnate the seed (*b*). And, consequently, that “*Omnium florum genuina consideratio, nec dicitur, nec confirmatur, generationem vegetabilium fieri*” in the manner the Sexualists pretend.

45. ALTHO’ I have already, perhaps, been too tedious, and said enough to overturn the modern doctrine of the sexes of plants; yet there still remain some arguments for it, which I cannot pass. For *Linnaeus* says, “*Anteras esse plantarum genitalia masculina, et earum pollen veram generaturam, docet essentia, praecedentia, situs, tempus, loculamenta, castratio, pollinis structura: stigmata, germini ubique adnexa, esse genitalia foemina, probat essentia, praecedentia, situs, tempus, casus, abscissio (c).*” Which in *Phil. Bot.* (*d*), and *Spons. Pl.* (*e*), are explained: but confirmed only, either by mistakes, or by false consequences. Thus,

“*Situs:*

(*b*) Vid. No 40. supra.

(*c*) *Lin. Fund. Bot.* § 143. 144.

(*d*) *P.* 90.

(*e*) *P.* 84.

“ *Situs* : Didynamistis stamina adscendunt
 “ sub corollae labrum superius, quo et se pi-
 “ stillum flectit.” But in the description of
 his class 14. or *didynamia*, “ Antherae sub
 “ labio superiore *saepius* reconditae” (*f*). And,
 concerning the *situs*, he adds, “ *Monaeciae*
 “ pleraeque flores stamineos supra pistilliferos
 “ gerunt : *zea*, *ricinus*.” But the *flores fla-*
minei are below the *pistilliferi* in *ricinus*.
 What he says of *tempus*, is answered above ;
 of *castratio*, below : and what he observes
 of the *loculamenta antherarum*, and *pollinis*
structura, teach nothing, but the author’s
 conjectures.

46. BUT, whether these arguments be
 conclusive or not, *castratio florum* most cer-
 tainly demonstrates the use of the *jarina foe-*
cundans, and consequently the sexes of plants?
 “ Veritatem hanc probat *castratio*. Si anthe-
 “ ras alicujus plantae uniflorae auferamus, et,
 “ ne aliqua alia ejusdem speciei adsit, cure-
 “ mus ; abortit fructus, vel saltem ova profert
 “ subventanea ; quod adeo certum, ut quis-
 “ que nullo non successu id experiri queat,”
Wahlbom (*g*). “ *Castratio* : *Melonis* flores sta-
 “ mineos

(*f*) Vid. Lin. Gen. pl. p. 261.

(*g*) Amaen. Acad. 1. p. 86.

“ mincos qui diligenter auferunt, fructus non
 “ obtinent. Tulipae solitariae si auferantur
 “ antherae ante casum pollinis, sterilis eva-
 “ det,” *Linnaeus* (b).

So the winds seem here to forget their du-
 ty. But, granting all here alledged, it proves
 nothing. For plucking off the *petala* may
 have the same effect; “ Saepius avulsis flo-
 “ ris foliis, antequam hiarent, in tulipa prae-
 “ cipue, expectavi an stylus incrementa ca-
 “ peret; et interdum ejusdem incrementum
 “ remorari observavi, quandoque quaedam
 “ femina, absque noxa, debitam sortita sunt
 “ magnitudinem,” *Malpighius* (i). Wounds
 also in otherways necessary parts, may be
 the cause of such barrenness, by depriving
 the seeds of their proper juices, &c. Never-
 theless there is reason to deny the fact: for
 surely M. *Wahlbom* did not make the experi-
 ment in every species of flowers: M. *Geof-*
froy's mays ripened some seeds, tho' castrated
 (k): and I made the trial in tulips, *nullo*
cum successu. Thus,

47. ONE

(b) Phil. Bot. p. 90. and 92.

(i) Oper. p. 70.

(k) Vid. Mem. Acad. 1711.

47. ONE year, observing two strong *tulips* growing together, in an inclosure surrounded with a tall and thick quickset *hawthorn-hedge*; I cut down two or three more *tulips*, which stood at some distance from them, so as to leave none within that inclosure, save the two I mentioned: out of these, gently opening the *petala*, I plucked all the *stamina* with their *apices* still intire. The consequences of this too rude *castration*, was a considerable extravasation of the juices, in the bottom of the flower, and a sudden decay of the *ovarium* or fruit, which never increased, but turned yellow, shrunk, and withered. In order to discover whether this abortion was owing to the wounds, or to the want of the dust of the *apices*; I suffered these two *tulips* to remain in the place where they were: and next Season, with the same precaution that no other *tulips* should flower within the inclosure, I opened the *petala*, and took out carefully, not the *stamina*, but only all the *apices*; which prevented any sensible bleeding of the parts. This more gentle *castration*, they bore perfectly well; the *ovarium* suffered nothing, in either of them, but

but increased, and came to maturity, quite full of seeds. See also *Gardener's* dictionary, article *generation*, near the end.

48. Thus I think I have sufficiently answered all the arguments for the sexes of plants, taken either from the structure of flowers, or experiments of any consequence that I could meet with. But since no small stress seems still to be laid on the *analogy* between plants and animals, as much favouring this doctrine; I must beg leave, a little to consider it also, altho' it is certainly true, to use Mr. *Needham's* words (1), that the method of reasoning by analogy, is but too apt to lead us into mistakes; and therefore we ought to be very diffident of consequences deduced this way: for mere analogy, founded on facts, and extended by conjecture, however plausible, can, at most, but furnish motives for a reasonable doubt, and further inquiry.

49. "OMNE vivum ex ovo; per consequens etiam vegetabilia: ovum, non foecundatum germinare, negat omnis experientia; adeoque et ova vegetabilium," *Linnaeus* (m). I shall not here enquire, whether either of these propositions are certainly true; but

(1) Phil. Transf. No 490.

(m) Fund. bot. § 132—150,

but only notice, that they neither prove nor explain any thing: for if *omne vivum* include vegetables as well as animals, as certainly, according to our author it does; then he might as well have said, *Omnia animalia et vegetabilia ex ovis, et per consequens etiam vegetabilia*: if *vivum* do not include vegetables, the consequence does not follow. The same holds in the second, and several others of his aphorisms.

50. In how many things soever plants and animals may agree, certainly they do not agree in every thing. “Ad summum, (says *Theophrastus*) (n) non omnia similiter atque “in animalibus accipi debent: nam (in plantarum genere) vis undique germinandi habetur; quoniam et undique animatum est.” And *Malpighius* (o), “In vegetantibus, ubi “non est tantus organorum apparatus, et “cuilibet sensibili particulae, omnia insunt “quae in toto deprehenduntur: nulla intercedente generatione, abscissi quicunque rami “frequenter in novam sobolem excrescunt; “vel naturae ministerio, deciduae minimae “et compendiariae plantulae, a tenellis surculis

(n) De hist. pl. l. 1. c. 1.

(o) Anat. pl. p. 76.

“culis sub seminum specie, propagationem
 “quocunque anno perpetuant.” Hence ma-
 ny plants are much better and easier propa-
 gated by cuttings, layers, offsets, gems or
 buds, than by seeds: hence *garlicks*, *onions*,
leeks, &c. carry gems frequently, on the top
 of the stalk, among, or in place of the seeds,
 as well as at their roots under ground. What
 are bulbuous roots but gems? and such *Caesal-*
pinus observed on the leaves of the *moly*; as
 they are frequently seen on the stalks of *tu-*
lips, *lilies*: and it is to buds that the increase
 of many trees is owing.

§1. Now, what is a bud? how does it
 differ from a seed? “Differt soboles a semi-
 “ne, ut foetus vivens ab ovo; semen enim
 “tanquam ovum est, in quo est principium
 “vitale, at vita nequaquam; soboles autem
 “vivit, primo quidem juxta parentem, ut
 “ejus germen, postea vero per seipsum, pro-
 “priis radicibus ex terra humorem trahens,”
Caesalp. de plantis (*p*): and *Malpighius*, (*q*)
 “Gemmae sunt velut infans, seu foetus ita
 “custoditus, ut suo tempore auctus, in sur-
 “culum excrefcens, tandem ova promat.
 “Erit

(*p*) Lib. 1. c. 5.

(*q*) Anat. Plant. p. 39. 77.

“Erit igitur probabiliter semen quasi gemma
 “pendula et decidua, alieno germinatura
 “solo.” And although the learned *Petrus*
Loefling, in his treatise called *Gemmae Arbo-*
rum (*r*), or rather the publisher himself, is
 pleased to say, “qui gemmam sibi represen-
 “tant ut alterum semen, fallunt et fallun-
 “tur” (*f*); yet his description of a *gemma*
 seems not altogether to agree with this *notan-*
dum: for (*t*) he says, “gemma est pars plan-
 “tae radici insidens, quae occultat squamis,
 “foliorum rudimentis, embryonem futurae
 “herbae.” And below (*u*), he explains what
 he means by *radix* thus, “Hae gemmae insi-
 “dent, vel radici sub terra reconditae, vel *ra-*
 “*dici supra terram in truncum ramosum assur-*
 “*genti*; illae, ubi carnosae fuerint atque mag-
 “nae, *bulbi* nomine veniunt, &c.” And else-
 where (*x*), “Gemmam proinde concipio in-
 “star herbae in compendium redactae, tectae
 “et contractae sua inter extrema folia, ut ab
 “aëris injuria conservetur; cui herbae nihil

M m

“amplius

(*r*) *Amaen. Acad.* 2. p. 182 224.

(*f*) P. 185. note k.

(*t*) P. 185.

(*u*) P. 186.

(*x*) P. 192.

“amplius deest, quam vis se extendendi; e-
 “am autem calor demum excitat.” Is not
 this as applicable to seeds, as to gems? “Se-
 men, (says *Linnaeus*) (*y*) proprie, novum ve-
 “getabilis rudimentum, humore rigatum,
 “vesica tunicatum.” And is not a *gemma*
 the same? or a more perfect seed? however,
 call it *embryon*, *compendium*, *primordium plantae*,
 or what you please, with its covers: since
Linnaeus owns, “*Generationes plantarum ex*
 “*semine et gemma esse coevas* (*z*); and “*gem-*
 “*mas, proinde ac semina, in se continere pri-*
 “*mordium plantae* (*a*);” it comes to the
 same thing for our purpose. For,

52. IF gems contain the *primordia plantarum*,
 they contain the most essential part of seeds;
 because the *seminis essentia consistit in corculo*
 (*b*); and the *corculum* is nothing but the
novae plantae primordium (*c*). Now since
 buds or gems are copiously produced by
 numberless plants, and often break out of
 the smoothest part of the bark, especially
 of pruned trees; and, since the smallest
 part

(*y*) LIN. Phil. bot. p. 54.

(*z*) Phil. bot. p. 88.

(*a*) Gem. arb. Amaen acad. 2. p. 185.

(*b*) Phil. bot. p. 56.

(*c*) Ib. p. 54.

part of a plant may be made to grow and emit gems, whether it be naturally fertile or barren, *male*, *female*, or *hermaphrodite*: does it not clearly follow, that neither difference of sex, nor *illapsus pollinis antherarum supra stigmata nuda*, are necessary to the production of the *primordia plantarum*, the essence of seeds, as well as of gems; and that no imagined analogy between plants and animals, can warrant or excuse the fullon and obscene names, imposed by the *Sexualists* on the different parts of the fructification of vegetables (*d*)?

53. NOT a few other arguments against the modern doctrine of the sexes of plants, might be brought from the structure of many compleat flowers, as well as from the numerous tribes of such as are called less perfect; some of which produce seed, but want flowers; others have neither flowers nor seed. For it requires more than an *ipse dixit* to prove, that “*omnis species vegetabilium flore et fructu instruitur, etiam ubi visus eadem*”

(*d*) Vid Cl. LINNAEI fundamenta botanica, § 140, 143, 144, et 146, and the learned commentaries on these, in the *Sponsalia plantarum*, and *Philosophia botanica*.

“dem non assequitur,” as *Linnaeus* asserts (*e*), I know he attempts to prove it thus (*f*), “*Muscorum semina Nos ; lemnae flores delineati a Vallisnerio ; fucorum flores observavit Reaumur ; pilulariae flores investigavit B. Jussiaeus ; fungorum stamina descripsit Michelius.*” And this is all, and affords but a lame proof, and mostly conjectural.

Thus, although *Dillenius* (*g*), in the end of a particular description of the *selago foliis et facie abietis*, *R. syn. p. 106.* or upright *ferr-moss*, says, “plura non observavi, nec semina in his vel calycibus, vel foliis bracteatis invenire potui:” yet because he adds, “Suspicio autem bracteas illas folia seminaria esse, et novarum plantarum productioni inservire;” the learned author of the *semina muscorum* (*h*), positively concludes, “Haec foliola, tempore autumnali matura decidunt a calyce persistente, et novam plantam propagant, radículas e basi exferentia.” But, since below (*i*) he owns, that “Seminaria muscorum tanquam nuda corcula, sine cotyledonibus,

(*e*) Fund. bot. § 139.

(*f*) Phil. bot. p. 89.

(*g*) Hist. Musc. p. 437.

(*h*) Amaen. acad. p. 2. 295;

(*i*) P. 299.

“cotyledonibus, sine tunicis animo concipi-
 “enda sunt.—Flos autem obscure nobis per-
 “cipitur, cum nullum *stylum*, nullumque *stig-*
 “*ma* habeat:” if, what he calls a seed, real-
 ly takes root and grows (for I find not that
 either he or *Dillenius* made the experiment);
 it has a better claim to the character of a
 gem, than of a seed. And, whether or not,
 it is evident that the “*generatio muscorum*
 “*minime fit, mediante pollinis antherarum*
 “*illapsu supra stigmata nuda.*”

54. THE same learned author (*k*) asserts,
 “*Quod pulvis in capitulis muscorum fit pol-*
 “*len masculum:*” that the “*semina lycopodii*
 “*officinarum, sunt purum putum pollen (l);*”
 tho’ others think them all rather seeds. But,
 whether they are either, I shall not, can-
 not determine: only they seem to resemble
 more the seed of the *lingua cervina*; which
 has been found fertile (*m*), without the foe-
 cundating influence of any *antherae* yet disco-
 vered, that I know of. But, granting the
 above cited aphorism to be true in its largest
 extent, it can never prove the alledged use of
 the

(*k*) P. 300.

(*l*) P. 293.

(*m*) Vid. II. ox, 3. p. 555.

the dust of the *apices* in imperfect plants, where it does not appear; since the contrary is demonstrated in the most perfect of the vegetable kingdom.

55. I noticed above, that several arguments might be taken from the fructification of some of the most compleat plants: and the author of the *semina muscorum* (n) gives some instances, and answers them; how satisfiyingly, I leave the reader to examine. Only, since there are there three species of the *viola*, I shall add a fourth, *viz. viola martia purpurea flore simplici odoro*, B. p. 199. or common *March violet*. This plant puts out its flowers with the *stamina* in *March*, on long foot-stalks; no fruit succeeds them, nor appears for some weeks, yea months after, when close on the root, and well covered with the leaves, the fruit is formed, soon ripens, and contains plenty of seeds; tho' all the flowers were plucked in *March*. Does either *tempus* or *proportio*, here confirm the sexual scheme?

56. To conclude; Had the modern doctrine of the generation of plants, continued only to influence the learned more accurately

to

(n) P. 186, &c.

to inquire into the structure of vegetables, I would never been at so much pains to confute it: but, since it has given occasion to an intricate deformation of botany, and to the introduction of an infinite number of new names, and perplexing, tho' childish terms, whereby this most useful science, is like to become not only vastly more difficult, but even ridiculous; I thought it high time to publish such arguments as to me appeared sufficient, by sapping the foundation, to overturn this hideous superstructure. For surely no method at all, is much better, than such an one, whose *nomenclature* is more difficultly acquired, than the knowledge of the plants themselves, which alone is true botany. But of this more elsewhere (o). It remains only that we inquire a little into the real use of the dust of the *apices*.

S E C T. IV.

56. AUTHORS are so much divided in opinion, as to the primary use of this dust, that it may be doubted whether it be of any to the plant which produces it. And, since vegetables serve only, or at least chiefly, for the use

(o) Tyrocin. Bot. 1. p. 40. 50.

use of animals; what the great Mr. Boyle says of the one, may well be applied to the other (*p*). And since we see bees frequently come out of flowers loaded with their dust, and apparently carrying it to their hives; it is not improbable that they feed on it. For, if Mr. Geoffroy's experiments can be depended on (*q*), it cannot be the *materia* of wax, without being digested and prepared in the insect; and, how many other insects may live on it, I cannot tell.

57. BUT,

(*p*) "The whole animal is but a part of that greater body the universe; and therefore cannot easily be supposed to have been framed and furnished with the parts it consists of, intirely for its own sake. And, when we say, that all its parts are contrived for the best advantage for the animal, I conceive it to be understood in this limited sense, that the parts are excellently framed for the welfare of the animal, as far furth as that welfare, is consistent with the general ends of the Author of nature, in the constitution and government of the universe. All which ends it is not an easy task to discover, tho' some of them may be investigable by us. And, it seems presumption to suppose, that the welfare of particular animals, is any further designed and provided for, than will consist with the cosmical ends of the universe, and the course of God's general providence, to which his special or particular providence, about this or that mere animal, ought, in reason, to be subordinated." Vid. BOYLE'S Works, vol. 4. p. 548.

(*q*) Vid. Mem. acad. k. 1711.

57. BUT, if it be insisted on, that its primary use must respect the plant itself: perhaps *Caesalpinus* may be in the right, with relation to the plants, whereof some, in the same species, carry *stamina*, and others the seed (*r*): and *Malpighius* (*f*), and *Tournefort*, with relation to such as bear both on the same individual plant: viz. that it carries off what is excrementitious, and unfit for nourishing the seed (*t*). I own, indeed, that this opinion does not well agree with that of *Caesalpinus*; it being more than probable, that the dust of the *apices*, in barren as well as fertile flowers, is of the same nature, and designed for the same use.

58. AND, from what has been observed of the *parietaria*, *ficus*, *viola*, &c. yea and from the general structure of fertile, as well as barren flowers ; it is also more than probable, that this dust is rather excrementitious and

N n noxious

(r) Vid. supra. No 6.

(f) Vid. p. 70.

(r) "Petala, (says Tournefort) alimentum a pediculo ac-
ceptum visceris instar, perficiunt, et fructui nascenti suppe-
ditant; ineptis humoris partibus per stamina, seu vasa ex-
cretoria, abeuntibus in apices seu receptacula. Diximus
jam apices, quicquid minus apti continet alimentum, in se
recipere, rerumque valvas a congestis excrementis deduci."
Inst. p. 69 and 70.

noxious, than useful to the nourishment, or foecundity of the seed. Nor is this inconsistent with its influence, on the production of the numberless varieties of new flowers, which yearly adorn the gardens of the curious: if the opinion, which has for some time prevailed, be well founded; to wit, that it is the best way to obtain new varieties of fine flowers, to plant near together some of the best kinds of the same species, differently variegated, and save their seeds: reckoning that the dusts of these flowers have such influence on one another, that their seed will produce finer varieties, than the seed of the best of them would do, if it stood alone, however otherwise managed. And on the same foundation it is, that M. *Du Hamel's* scheme, for producing new varieties of fruits, is built (*u*). I say, allowing all this to be fact, which I cannot confirm; it seems rather to establish, than to be an objection against Mr. *Tournefort's* opinion.

59. FOR, since all double flowers are monsters, “*Luxuriantes flores* (says *Linnaeus*) “*nulli naturales, sed omnes monstra sunt;* “*pleni eunuchi evaserunt, proliferi monstro-* “*forum* “*(u) Vid. Mem. acad. an. 1728.*

“*forum augent deformationem (x)*” ; since the finest varieties of flowers are the most tender ; since variegation of flowers, as well as of leaves, is preternatural ; and since even the improvements of plants, or fruits by culture, with relation to their usefulness to us, are but diseases in the plants, considered in their natural state : may it not be properly inferred, that, whatever influence the dust of the *apices* has in producing these, it must be owing to its noxious, not foecundating vapours ?

60. AND, to conclude, although it should be granted that we have not sufficient *data*, to enable us to determine positively of what real use this dust is to the plant : yet, since it has appeared, to a demonstration, certain, that fertile seeds may be produced without it ; our ignorance herein, can never be an argument of any consequence, for the modern sexual scheme, or doctrine of the sexes of vegetables, against which I have been arguing ; with what success, I submit to the learned to determine.

(x) Fund. Bot. § 159.

ART. X.

*Remarks on chemical Solutions and Precipitations, by ANDREW PLUMMER, M. D., Fellow of the Royal College of Physicians, and Professor of Medicine and Chemistry in the University of Edinburgh. **

THAT I may not give the first instance of breaking in upon the order and rules established in this society, I shall lay before you, at this time, some miscellaneous observations, on the solutions and precipitations which frequently occur in the prosecution of chemical enquiries.

I shall lay down these remarks by way of propositions, and, after each, shall mention the facts or experiments which gave occasion to the remark, or which confirm and illustrate the proposition.

ALTHOUGH many of the instances which I am to adduce, are common, and well known to every one versant in chemical experiments; yet, as the history of chemical solutions

* January 3d 1738.

solutions is very extensive, and as many singular conditions and appearances, are to be observed in the actions of various solvents, upon the bodies which they dissolve; it may possibly be of some advantage, to any one who shall afterwards undertake to give a rational and satisfactory account of the causes which produce such effects, and of the laws by which these causes act in particular circumstances, to have in his view a great many inferior axioms or canons, comprehending as many particular instances as possible: because these may enable him to ascend to more general canons; and at length to the ultimate physical cause; and again, from thence to explain the *phænomena*, in particular instances.

By *solution*, is here meant, the separation and division of a solid body, or of a thick and consistent mass, by means of a sensible fluid, into parts so small, as to disappear and be equally dispersed through the fluid.

THAT liquor which makes the separation of parts, I call the *solvent*; or, in the stile of the Chemists, the *menstruum*.

I must take the liberty, to make one preliminary remark, before I descend to particulars;

lars: for, if the assertion of certain Chemists was well founded, it would, in a great measure, supersede any further enquiries.

I. THE notion of an universal *menstruum*, capable to dissolve equally every body to which it is applied, appears repugnant to the nature of things, and to these limited powers which we discover in all known bodies.

I shall not undertake, in this place to adduce and examine the bold assertions and high pretensions of some Chemists to the discovery of an universal solvent; because it would exceed the limits which I propose to myself, and perhaps afford little entertainment or instruction. But I must be allowed to doubt of the possibility of an universal solvent, until more certain evidences are produced than what have hitherto appeared. For, as there is a vast variety of bodies which differ much from one another in density, solidity and texture, in the bulk, shape and composition of the constituent particles, in the degree of force with which these cohere among themselves, and in the number, size, and figure of the pores or interstices betwixt the solid parts; it is scarce conceivable,
that

that any one liquor can be endued with powers corresponding to all the various circumstances of such a variety of bodies, so as indiscriminately to dissolve all : it is therefore much more agreeable to the nature of things, that there should be a variety of liquors endued with different qualities, in different degrees, to separate the particles of so many different solid bodies ; and daily experience sufficiently confirms the truth of this conclusion. There is perhaps no body, natural or artificial, but there may be found a *menstruum* proper to dissolve it : and, tho' some liquors are known to dissolve several bodies which differ in many respects ; yet even these solvents, which are allowed to be most extensive, are found incapable of dissolving a great many other bodies.

II. THE density, solidity, hardness, fixedness and other manifest qualities of bodies, can afford us no certain mark or indication of the qualities of the liquors, requisite to dissolve them, if experience is wanting. Neither can we conclude, from the strength, sharpness or corrosiveness of liquors, discovered by their effects on the human body, the

the fitness of such liquors to dissolve different substances.

IRON and copper, the hardest of the metals, which require the greatest force to extend them, and the strongest fire to bring them to fusion, will be corroded and dissolved by liquors most harmless to the human body; as vinegar, juice of lemons, a solution of tartar, rhenish wine, nay moist air: whereas quick-silver, whose particles are so mobile, easily separable and dissipated by a moderate heat, and tin, which is very soft and easily melted, require more corrosive *menstrua*. Coral, and some of the hardest gems, will be better dissolved in a weak vegetable acid, than in a much stronger one of the mineral kind. *Oleum vitrioli*, *spiritus nitri*, or *aqua fortis*, the most ponderous and corrosive of the acid liquors, cannot dissolve bees-wax or crude sulphur, which will readily relent in the mild and soft oil of olives; *sal tartari*, or *oleum tartari*, *p. d.* is almost equally corrosive, with regard to the human body, as *aqua fortis*: yet the solvent powers of these liquors, with respect to other substances, are most opposite.

III. THE

III. THE fitness of a liquor to dissolve a certain body, cannot be certainly learned from the agreement of that body in several properties with another, which we know can be dissolved in that liquor. Neither can we safely conclude, that two liquors, which nearly resemble one another, will mutually dissolve all the same bodies.

WE meet with abundance of instances, in chemistry, to make us cautious in drawing such conclusions. Well rectified spirit of wine, makes a very compleat solution of colophony or any of the common rosins; but there are several substances which have a great affinity to rosins, as *lacca*, *copal*, *amber*, *sulphur*, which being digested with highly rectified spirit of wine, will scarce give a tincture to the spirit; that is, these bodies remain either altogether, or, for the far greater part, indissoluble in that liquor, unless they are previously prepared and disposed to a solution, by the interposition of another body, as a fixt alkaline salt, &c. Destilled vinegar will readily dissolve copper, lead, iron; but, it would be rash to conclude, that the same liquor will also dissolve gold, silver, antimony, whatever agreement we find between these bodies

and the former. On the other hand, *aqua fortis* and *aqua regia* appear to be much of the same nature; and the former, by a very small change, may be converted into the latter; yea, further, they dissolve equally several bodies, as copper, iron, spelter, tinglass: yet, it would be erroneous to conclude, that, whatsoever body one of these liquors dissolves, the other would dissolve the same; for *aqua fortis* dissolves silver and not gold, and, *vice versa*, *aqua regia* dissolves gold and not silver.

IV. THE solvent power of some liquors, with respect to certain bodies, may be increased, diminished or destroyed, by a very small and almost imperceptible change of the liquors.

I put two grains of gold into a drachm of *spiritus salis marini*, and set the glass on warm sand; after some hours, I could observe no sensible change of the bits of gold, in colour or bulk, nor of the liquor: then I put into the glass, as cautiously as I could, one drop of *spiritus nitri*; after some time, I found the liquor had assumed a bright yellow, or golden colour, and the gold quite dissolved; for there remained only a few white particles
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at the bottom, which were probably silver. Thus the very small addition of *spiritus nitri* very much increased the solvent power of the *spiritus salis marini*. It is known, that *oleum vitrioli* poured on quick-silver, and kept in the common air, or in a very gentle heat, does not corrode or dissolve it; but, if they are brought to a heat exceeding that of boiling water, then the solution takes place. Mr. Boyle testifies, that, by his care to rectify or dephlegmate *aqua fortis* very much, he rendered it unfit to dissolve silver. While *aqua fortis* is dissolving copper with a brisk motion, if a little *alcohol* is poured in, the solution will be stopt.

V. SOME bodies which we know can be easily dissolved in certain liquors, may be so changed by the fire, or otherways, that the same liquors will not afterwards dissolve them. On the other hand, the fire renders some bodies fit to be dissolved in certain liquors which could not be dissolved in them before.

THE oil of vitriol, as I said before, assisted by a proper degree of heat, readily enough dissolves quick-silver: but when quick-silver is, by long digestion in a moderate heat, deprived

deprived of its fluidity, and turned into a red *calx*, or has been first dissolved in *aqua fortis*, and afterwards, by evaporation and calcination, freed from the acid; and reduced to a red powder; if either of these red powders, I say, is digested with oil of vitriol, and made to boil, this liquor will not now dissolve the mercury, or so much as change its colour. On the other hand, tho' *spir. sal. mar.* is not esteemed a proper *menstruum* of quick-silver in the immerfive way, but when applied to it in form of vapour in sublimation; yet if this spirit is poured upon the red precipitate mercury, it presently changes its colour to white, and quickly dissolves it. Again, *spir. sal. mar.* readily dissolves clean and fresh filings of iron: yet if these are frequently moistened with water, and by trituration and calcination turned to a *crocus*; *sp. sal. mar.* poured upon this, raises an effervescence indeed, but does not seem to make a solution; for the iron lies at the bottom not sensibly diminished and the colour of the liquor is not changed. Some ores, as they are taken from the mine, tho' they appear pretty rich and clean, yet will not readily dissolve in the same *menstrua*, which are fit to dissolve the pure metals, un-
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til the ores are roasted, washed and smelted. The *calces* of lead and tin will more easily dissolve in spirit of vinegar, than the thin plates or shavings of the same metals.

VI. SOME liquors which dissolve several different bodies, while they perform the solutions, seem to act in a different manner upon each body, require different helps, and dissolve different quantities of each.

THIS is very remarkable in the action of oil of vitriol upon iron and quick-silver: to make oil of vitriol dissolve iron successfully, it must be diluted with a quadruple quantity of water; when the clean filings are put into this liquor by degrees, there arises a strong ebullition and effervescence, with a considerable heat and thick steams of a penetrating sulphurous smell, which are apt to kindle and make a fulminating noise upon the approach of the flame of a candle; and, lastly, one ounce of oil of vitriol thus diluted will dissolve an equal weight of pure filings of steel: but, when oil of vitriol is employed to dissolve quick-silver, it must be strong and well dephlegmated; when it is poured upon the quick-silver, it raises no intestine motion or heat; it does not begin to corrode or dissolve the

the quick-silver, till there is such a heat applied, as will almost bring the liquor to boil, and even then the solution goes on slowly and quietly: lastly, to dissolve one ounce of quick-silver, it requires, at least, three ounces of oil of vitriol; and yet the quick-silver will not be entirely suspended in this ponderous liquor.

EVEN water, tho' it dissolves all kinds of salts, yet it does not act upon all of them with the same ease; that is, the same quantity of water cannot dissolve an equal quantity of each salt, in the same time, or with the same degree of heat; for an ounce of cold water will soon dissolve half an ounce of salt of tartar; but half an ounce of crude tartar cannot be dissolved in less than ten ounces of water; and that quantity of cold water will make no solution until it is brought to boil, and continues to boil for some time, new water being added to supply the waste. Even that compound salt, made of the fixt salt and chrystals of tartar, which, from its solubility, with respect to common tartar, gets the name of *tartarus solubilis*, requires little less than ten times its weight of warm water to dissolve it fully.

VII. SOME

VII. SOME bodies can be dissolved in liquors of very different qualities, but in different proportions, with different helps, appearances and effects.

COMMON sulphur or its flowers dissolve in any oil, but more easily, and in greater quantity, in a thick, mild, exprest oil, than in a thin, hot, aromatic, distilled oil. When flowers of sulphur are digested with oil of olives, or lint-seed, it becomes a thick balsam; three ounces of oil will dissolve one ounce of sulphur; and any small part of sulphur which remains, has the appearance of melted sulphur. But when the flowers are digested with æthereal oil of turpentine, six ounces of oil will scarce dissolve one of sulphur, and what remains is not like the sulphur in the former case; for, if the vessel is cooled gradually before the balsam is poured off, the sulphur appears almost like nitre when it chrySTALLIZES, or a vegetating salt branched out into long slender stalks. Sulphur likewise dissolves in spirit of hartshorn, but still in less quantity, and with a smaller degree of heat. And it is to be observed, that the unrectified or oily spirit will dissolve more sulphur than the clear and pure spirit.

spirit. To make sulphur soluble in water or spirit of wine, it must be melted, then mixed and incorporated with a fixed alkaline salt, by which means it will mostly dissolve in warm water, and give a deep and strong tincture to spirit of wine.

THE *calx* of lead, *litharge* or *minium*, dissolve in distilled vinegar; but more fully and quickly in *aqua fortis* diluted with six times its weight of water. The solution of lead in spirit of vinegar, being evaporated slowly, to a due consistence, and then removed to a cool place, never shoots into fair distinct chrystals; but thickens, as it were, into a saline mass, like coarse sugar, somewhat moist; and, with difficulty, can be brought to a tolerable whiteness: but the solution in diluted *aqua fortis*, treated in the same manner, gives large, solid, shining white chrystals, regularly shaped. The *calx* of lead or *minium* will likewise dissolve by digesting or boiling in oil of olives or lint-seed; but as it dissolves, it thickens into the consistence of an unguent or plaister.

Good *aqua fortis* or *spiritus nitri* will dissolve about an equal weight of quick-silver, and the solution is performed with a great commotion

commotion, heat, and thick red smoke : strong oil of vitriol dissolves scarce a third part of its weight of quick-silver, and the solution is made in a slow and imperceptible manner, and requires the assistance of a great external heat.

THE manner in which the acid of sea salt is applied to quick-silver, in order to dissolve and be united to it, is still more extraordinary : there are two methods practised, but both depend on the same principle, and produce the same effect. For either the quick-silver is rubbed in a marble mortar with green vitriol calcined to whiteness, decrepitate sea salt, and a small proportion of dry salt-petre, till the quick-silver is extinguished and disappears, this mass is put into a subliming glass, a gentle heat is made at first, and gradually increased ; in the beginning arise white steams, if these are collected, they condense into a liquor, which proves a weak *aqua regia* ; then there will sublime a white shining chrystalline body, which is a vitriol of quick-silver, and goes under the name of *mercurius sublimatus corrosivus* : that the quick-silver is here dissolved by, and united to the acid of sea salt, with a small quantity of the nitrous a-

P p

cid,

cid, is evident, because the same materials, without the addition of quick-silver, properly treated, yield a good *aqua regia* which dissolves gold. Or quick-silver is first dissolved in *aqua fortis*, the solution is evaporated till there remains a white, dry, vitriolic body, which is to be mixed and rubbed with an equal weight of decrepitate sea salt, and sublimed as before: here likewise the acid of sea salt, with a portion of the nitrous, is joined to the mercury; for one way of preparing an *aqua regia*, is by pouring *aqua fortis* upon decrepitate sea salt, and drawing off a liquor by distillation, which is in effect done in this case, only the acid incorporates with the quick-silver into a vitriolic form.

VIII. THE solvent powers of certain *menstrua* are not destroyed in the act of solution, but can be exerted on other bodies; and the *menstrua* separated from the dissolved bodies, may retain or recover all their former qualities.

THIS property of several solvents, will be found to obtain, in many more instances than is commonly imagined. Many solvents, when they are actually dissolving bodies, excite a strong and remarkable effervescence, with a great expansion, hissing, bubbling, heat and smoke;

smoke; and, after the solution, the liquors frequently acquire new qualities which were not conspicuous before, either in the solvents or in the bodies dissolved: whence it is generally imagined, that, from this struggle or action and reaction betwixt the bodies, the particles of the *menstruum* are broken, their points blunted, and their figures changed, by penetrating into the pores of the solid body, by disjoining and dividing the parts very minutely. But, I shall endeavour to make it appear, from experiments, that the active particles of most liquors, employed as solvents, are extremely tenacious of their figure, bulk and density, at least of the powers and properties which depend on these, and constitute the particular marks and characters of these liquors: that, tho' they have dissolved several different bodies, have entered into many compositions, and have past thro' a great variety of forms; yet, after all, most of them can be brought back to their primitive state, and made to exert the same powers which they did at first, and again run thro' all the successive changes; so that it may be doubtful whether art can

intirely

intirely change the nature of some of these solvent liquors.

To begin with some familiar instances to confirm our remark. Water can dissolve a certain quantity of any salt; yet, when it is fully impregnated with one salt, it will still dissolve a portion of another salt, tho' not so much as the same quantity of pure water would do. From sea water, by evaporation and chrySTALLIZATION, are procured three distinct salts, besides other substances, as the ingenious Mr. *Brown* has observed, viz. the muria-tic, or salt for common use, the bitter pur-ging salt, and a fiery calcarious salt, different from the other two; yet sea water will dissolve, at the same time, a pretty consider-able quantity of several other salts, as sugar, salt ammoniac, salt petre and salt of tartar; and, after it has dissolved sufficiently of two or more of these salts, it will, even then, dissolve some more sea salt, and still more of the epsom or *Glauber's* salt. Hence, by the way, we find such a variety of mineral or medicinal waters, impregnated in various proportions, with different salts, vitriols, met-als, earths, &c. The acid of sea salt, which has dissolved quick-silver, and chrySTALLIZED
with

with it in the sublimation of corrosive mercury, will dissolve the metallic or reguline part of antimony into a liquor, called *butyrum antimonii*; and this will afterwards dissolve gold. Dissolve silver in *aqua fortis*, when the solution is finished, add to it some crude salt ammoniac, in powder, and set the vessel in hot sand; the silver will all fall to the bottom: then pour the clear liquor into another glass; this will be found a good *aqua regia* fit to dissolve gold, *regulus antimonii*, &c. There is another noted experiment which evidently illustrates and confirms the remark. If fine copelled silver is dissolved in proof *aqua fortis*, and the solution is diluted with an equal quantity of distilled water; it will continue clear and limpid, without any precipitation: if then a piece of polished copper is put into the solution, the silver falls gradually down, and the copper is dissolved, as appears by the green colour which the liquor acquires in a little time. When the silver is all separated, take out the remaining copper, and put in a piece of clean iron; the green colour goes gradually off, by the copper falling down, and the iron is dissolved in its place: after allowing a
sufficient

sufficient time for the solution of the iron, and precipitation of the copper, put into the solution a piece of lime-stone; the *aqua fortis* immediately works upon it with fresh vigour: lastly, add some salt of tartar, or *oleum tartari, p. d*; this precipitates the stoney matter, and renews the effervescence. This seems to finish the work, and quite to absorb the acid, and destroy all its solvent powers: yet even here the acid only lurks and conceals itself by its strict union with an opposite salt. But art can again produce it to view, and restore all its former powers. If in this process, five or six ounces of *spir. nitr.*, or *aqua fortis*, were employed in the beginning, and, after the last step, the liquor is diluted with some more water, strained thro' brown paper, fully satiated with the alkaline liquor or salt, and then slowly evaporated and chrystallized; there will be procured a salt perfectly resembling common salt-petre, in the shape of the chrystals, explosive quality, and other marks. This, factitious salt-petre, then may be employed in place of the common; and I distilled some ounces of a salt, made much in the same way (for I imagine the intermediate steps will

will make no alteration) with oil of vitriol, in *Glauber's* manner, and obtained a strong smoking spirit, which answers all the characters of *Glauber's* spirit of nitre, and dissolves the same bodies; and therefore may run thro' the same course, over and over, without any change, unless that it may be gradually wasted, some part of it being dissipated in every operation.

I found the same thing hold in other acid liquors, as spirit of sea salt, spirit of vinegar, the acid of tartar, &c. which I joined to alkaline salts, and recovered the acid by distillation. These and some other experiments of the like nature, which I have not yet had leisure to consider in their full extent, I shall take another opportunity to lay before the society. In the mean time I shall proceed to make some

Remarks on Precipitation.

By precipitation is meant the parting or separating the small particles of a dissolved body from the solvent liquor by means of a third body solid or fluid, which is therefore called the precipitant.

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THERE is indeed another manner of parting the dissolved body from the solvent, which is sometimes, tho' less properly, called precipitation; when, by drawing off part of the solvent liquor by evaporation or distillation, the dissolved body falls down gradually in proportion to the quantity of the liquor wasted.

I shall chiefly consider the first or more proper precipitation, in which there is both greater variety, and more difficulty to account for the change.

Remark I. ALL precipitations do not happen from the opposition of an *alkali* to an *acid*; but there are different means of precipitating different solutions.

IT is certain, from experience, that whatever bodies are capable of being dissolved in any acid liquor, may be precipitated from it by an alkaline salt or liquor. On the other hand, any body that is dissolved in an alkaline liquor may be parted from it by the addition of an acid. This then is a pretty general method of precipitating: however this opposition of acid and alkali is not the sole cause of precipitation; for many metals and metallic

metallic substances dissolved in their proper acid *menstrua*, may be precipitated by other bodies that are not alkaline, some by plain water, some by neutral salts, natural or artificial; some by another metal, some by another acid liquor; and, lastly, some of these solutions may be precipitated in two or three different ways; of all which I shall give instances afterwards. But further, acid and alkaline liquors are not the only solvents; for water, ardent spirits, oils, and several compound liquors, neither acid nor alkaline, dissolve a great many different bodies, which likewise may be separated from them by precipitants of different qualities. Therefore an acid and alkali do not always concur to make a precipitation.

II. THE weakening or diluting solutions, by a liquor specifically lighter than the solvent, is not the sole cause of precipitation.

SOME who would account for precipitations upon mechanical principles, contend that diminishing the specific gravity of the solvent liquors is the cause of all precipitations: but this, altho' it obtains in some instances, yet, in many more, it will be found contrary to experience. If any salt is dissol-

ved in water, and if spirit of wine, a liquor specifically lighter than water, is added to the solution; no precipitation will ensue.

WHEN silver is dissolved in good *aqua fortis*, the solution may be diluted with fair water to any degree you please; yet the particles of silver continue suspended in the liquor, tho' rendered specifically lighter: whereas, when the thinnest and lightest parts of the solvent are carried off by evaporation, the silver falls down. In the same manner, a solution of quick-silver in *aqua fortis*, a solution of gold in *aqua regia*, of lead in spirit of vinegar, and some other solutions of metallic substances, may be diluted with simple water without precipitation. Further, sometimes a liquor specifically heavier than the solvent is added to the solution, whereby the specific gravity of the compound liquor is increased; yet the particles of the dissolved body are no longer suspended in it, but fall to the bottom, quite contrary to this hypothesis. When rosin of jalap is dissolved in highly rectified spirit of wine or *alcohol*, if water, which is specifically heavier than the spirit, is poured in, the resinous particles can no longer be sustained. The sublimate corrosive
mercury

mercury can be dissolved in lime-water or common water : upon pouring in oil of vitriol, a very ponderous acid liquor ; the mercury subsides. Coral dissolved in spirit of vinegar, will be thrown down by the same oil of vitriol. These instances naturally lead us to another remark.

III. SOME bodies dissolved in one acid liquor may be precipitated by another acid.

To the two former examples of this manner of precipitation, I shall add some others. Silver, dissolved in *aqua fortis*, will be thrown down, if *spir. sal. marini* is put into the solution : on the other hand, a solution of gold in *aqua regia*, suffers a precipitation by the affusion of *spir. nitri*. That corrosive acid liquor, called *butyrum antimonii*, is known to be a solution of the metallic part of antimony in *aqua regia* : if we pour into it *spir. nitri*, after a pretty strong effervescence, we shall find a metallic powder at the bottom.

IV. SOME metals dissolved in an acid liquor, may be precipitated thence by another metal or metallic, stony or calcarious substance, capable of being dissolved in the same *menstruum*.

UNDER

UNDER the head of solution, I had occasion to adduce a well known instance, to show that a *menstruum* having dissolved one body, will successively dissolve several others in a certain order. I am obliged to mention the same again, as a confirmation of the present remark. If pure silver is dissolved in proof *aqua fortis*, and the solution diluted with fair water; the silver will be precipitated by copper, the dissolved copper by iron, and that by lime-stone. This however is not the sole instance of this way of precipitation: others may be found. The sublimate corrosive mercury may be justly looked upon as a solution of quick-silver in the acid of sea salt, or *aqua regia*, brought into a chryselline or vitriolic form by sublimation: when this corrosive mercury is mixed with an equal weight of common antimony, and these rubbed together, left to moisten in the air, and then distilled; there happen such changes to this mixture, as evidently show a real precipitation of the quick-silver by the reguline part of the antimony; for the acid forsakes the quick-silver, and dissolves the *regulus*, which makes the *butyrum antimonii*; and the quick-silver freed from the acid, is by the fire incorporated

incorporated with the sulphur of antimony into a black powder like *Æthiops mineralis*, which may be sublimed into the *cinnabaris antimonii*. Quick-silver dissolved in *aqua fortis*, may likewise be precipitated by another metallic body, as zink or bismuth.

V. SOME bodies dissolved in their proper *menstrua*, may be precipitated thence, by several bodies of different qualities.

WE meet with a great many instances in chemistry to confirm this remark. I shall mention a few examples, in which the greatest variety obtains. Pure silver being dissolved in good *aqua fortis*, and the solution distributed into six or eight glasses, we may make as many precipitations by different precipitants; for solutions of sea salt and crude salt ammoniac, spirits of sea salt, vitriol and salt ammoniac, a fixt alkaline salt, or its lee, and a piece of copper, will, each of them separately, make a precipitation of the silver.

QUICK-SILVER dissolved in *aqua fortis*, may be precipitated by a solution of sea salt, by a solution of crude salt ammoniac, by fresh urine, by spirit of salt ammoniac, by *ol. tart. p. d.* and by zink.

REGULUS

REGULUS of antimony dissolved in *aqua regia*, will be precipitated by plain water, by a solution of salt-petre, by spirit of nitre, and, by the alkaline liquors, fixt and volatile.

VI. THE matters precipitated from the same solution, by different precipitants, differ from one another in several remarkable qualities.

WHEN either a muria of sea salt, a solution of salt ammoniac, or spirit of sea salt (for these three have nearly the same effects) is put into a solution of silver; it becomes milky, and, as it were, cruddled, and, at length, a white powder subsides to the bottom: this powder being washed with warm water and dried, is soft and impalpable; its weight exceeds that of the silver dissolved, by more than one sixth part. This powder comes much sooner to fusion than silver, but does not recover the appearance or properties of that metal; for it looks like a piece of yellowish glass, semi-opaque and brittle, yet bending or yielding a little, whence it gets the name of *luna cornea*, or horny silver; and it is with great difficulty that it can be reduced to true malleable and ductile silver.

WHEN

WHEN a plate of copper is put into a solution of silver diluted, the surface of the copper is immediately covered, as it were, with a coat of silver; this becomes gradually thicker, and appears tufted, spongy and loose: upon shaking the plate, large pieces of this crust falls off, which either rise to the top, or swim about in the liquor for a while, till discharging many bubbles of air, they grow heavier, and fall down in form of a white powder; the silver being in this manner separated from the liquor, then washed and dried, is nearly of the same weight with the silver dissolved; and, when melted, runs into a lump of pure malleable silver, without any considerable loss.

If an alkaline liquor, as *spir. sal. ammoniaci*, or *ol. tartari p. d.* is dropt into a solution of silver, it excites a strong effervescence, which will be renewed when more of the alkaline liquor is dropt in at intervals, until the acid is satiated; and there appears a white, bulky and frothy matter, which gradually subsides: all being thrown into a cap of brown paper, fitted to a funnel, and warm water poured in, the frothy matter diminishes considerably in bulk; and there is left a powder

powder of an ash colour, which being dried, a little exceeds the weight of the silver dissolved.

THE precipitations of mercury shew still a greater variety, at least, as to their effects, being used as medicines. If sea salt, dissolved in a good quantity of water, is put into a solution of quick-silver in *aqua fortis*, it turns the clear and limpid solution turbid, opaque and milky; at length the white matter subsides, which being separated from the fluid, washed and dried, remains a very white powder, and a little exceeds the weight of the quick-silver dissolved: but it is to be observed, that the *muria* does not precipitate all the mercury; for the liquor, thus separated from the white powder, is known to have the effects of a cosmetic or deterfive mercurial water; besides if *ol. tart. p. d.* is dropt into this liquor, a small quantity of a brown mercurial powder will be precipitated. Physicians know that this white precipitate is a pretty active and brisk medicine, and produces greater effects, even in a smaller dose, than *calomel*, but is more mild and tractable than the *merc. sublim. corrosivus*; for, when this last is somewhat tempered by *spir. sal. ammon.* it becomes

comes a medicine much of the same nature with the preceeding white precipitate. A solution of crude salt ammoniac, and *spir. sal. mar.* have the same effects on the solution of quick-silver, as the *muria* has.

If a little fresh urine is put into a solution of quick-silver in *aqua fortis*, there will likewise happen a precipitation; but the powder is not white, but of a pink or fleshy colour, it appears pretty bulky, and has acquired a little additional weight. This *precipitate* is not usually prescribed by physicians; but M. *Lemery* pretends that it is milder than the white, and only operates by stool.

It would appear then, that, in many instances of precipitation, the particles of the dissolved body are increased in density and specific gravity; by reason that some parts either of the precipitating matter only, or of the compound, made up of the solvent and precipitant, are very firmly united to them, so that they cannot longer be sustained in the liquor. But, in other cases, where the matter precipitated has acquired no addition of weight, no new or remarkable properties, and is easily reducible to the native form of the body dissolved; there must be some o-

ther cause of precipitation, which may probably be, that there is a greater affinity betwixt the precipitating body and the solvent, than betwixt this and the body dissolved ; whence, from the disposition of these bodies to unite, they shake off, or drive away from them, the particles already joined to the solvent.

A R T.

ART. XI.

Experiments on Neutral Salts, compounded of different acid Liquors, and alkaline Salts, fixt and volatile; by the same.

* **A**T a former meeting of the society (January 1738) I read some remarks on chemical solutions; the last of which gave me occasion to mention some experiments, in which I was then employed, but had not finished: and therefore, at that time, I could not consider them in their full extent, or have a just view of all the consequences that might be drawn from them.

I now proceed to relate the experiments, and to observe how far they may be useful to extend our knowledge.

I prepared a parcel of factitious, or (as the Chemists are pleased to call it) regenerated nitre, in this manner; I took two ounces of an extemporaneous fixed alkaline salt, made of equal parts of nitre and tartar; I dissolved

* June 7. 1739.

diffolved this salt in warm water, filtered the solution, and poured into it strong spirit of nitre, a little at a time, until the alkaline liquor was perfectly satiated with the acid, which required one ounce and a half of the spirit of nitre. From this saline liquor, by evaporation and chrySTALLIZATION, I procured two ounces and a half of salt, when it was all collected and well dried. The chrySTALS were long, slender and prismatical; the salt had a pretty mild and cooling taste; and bits of it put upon a burning coal, made it sparkle and give a bright flame.

WE may observe by the way, that the compound salt exceeded the weight of the fixed salt by half an ounce or 240 grains; this additional weight it procured from the spirit of nitre; therefore one ounce and a half, or 720 grains of the spirit of nitre which I used, contain 240 grains of true acid, which is capable to satiate four times its weight of alkaline fixt salt.

I put two ounces and a half of this factitious nitre into a small glass retort, and poured upon it one ounce of oil of vitriol, which immediately begun to raise an ebullition with heat and red fumes; a receiver was instantly
joined

joined to the retort with paste; the glass set in a sand furnace, and the fire increased gradually. After the operation, there was found in the receiver, a smoaking acid liquor, of an orange colour, which weighed one ounce, two drams, three grains: there remained at the bottom of the retort, a cake of white solid salt; which being dissolved in warm water, the solution filtered, and part of the water again exhaled, shot into chrystals; and these being all carefully collected, weighed two ounces, twenty two grains.

THE spirit obtained in this manner was fiery, active, and constantly emitting red fumes; it dissolved silver, kindled with oil of cloves, and agreed in all other characters with strong spirit of nitre. The salt which remained in the retort when purified and chrySTALLIZED, no way resembled salt-petre, or the salt employed in this operation, in the shape of the chrystals, taste, or other properties; for these chrystals were polyhedras, having several triangular faces and solid angles; their taste is rough, bitter, and somewhat astringent; they do not readily melt or sparkle in the fire, as common or factitious nitre does,

I made another factitious salt, by impregnating a solution of the same alkaline fixt salt with spirit of sea salt, pouring it in by degrees, till there appeared no effervescence, and the liquor tasted briny. Four ounces and a half of this spirit were employed to satiate two ounces of the fixt salt : when the two saline liquors were duly proportioned, there appeared, before evaporation, a great many small grains of white salt at the bottom, which, as well as the liquor surrounding them, had the true muriatic taste.

THE salt obtained out of this liquor, had the genuine figure of the common sea salt; but the chrystals were smaller, more hard, white and opaque : the salt is not altogether so pungent as what we commonly use ; but more agreeable and cooling, yet without any acidity. This salt, while moist, weighed two ounces, six drams and a half ; but, when well dried, and slightly decrepitate, the weight was reduced to two ounces and two drams. I put this quantity of powdered salt into a small glass retort, and poured upon it two ounces of strong spirit of nitre, which occasioned an ebullition with heat and smoke. I proceeded to the distillation, with a fire gradually

dually increased; when the vessels were cooled, I found in the receiver, two ounces four drams and a half, of an acid liquor, of a faint greenish colour, which smoked much when the vessels were first disjoined; but the fumes were paler and whiter than those of spirit of nitre, and sooner ceased. I put a few drops of this liquor into a solution of silver in *aqua fortis*; it made it white and cruddled, and at length a white powder fell down. I poured a small quantity of it upon a few grains of filings of gold; with the assistance of a little heat, the spirit pretty quickly dissolved it, leaving only a few particles at the bottom. From these characters I think we may conclude, that the acid liquor, produced in this operation, is a true *aqua regia*: I chuse rather to call it so, than spirit of sea salt; because I found it had a greater and quicker effect in dissolving gold than the plain spirit of salt had, therefore it is probable, that a small quantity of the nitrous acid had accompanied the acid of sea salt.

THERE remained in the retort, a cake of solid white salt, like melted nitre, or *sal prunellae*: after it had been dissolved in warm water, the solution filtered and evaporated; there

there appeared many regularly shaped chryftals, long, slender, and prismatical, of the nitrous kind ; which being removed, and more of the moisture exhaled, the chryftals which formed were not so large nor so regularly shaped as the preceeding, but the whole appeared like a mass of white salt coarsely powdered : however I could discover, among these chryftals, some that had a quadrangular base, but long sides, like two cubes joined; others variously compounded or broken : even this part of the salt had the cooling nitrous taste ; and, part of it put upon a live-coal, sparkled and excited a bright flame like salt-petre.

AGAIN, I took two ounces of that neutral salt, called *tartarus solubilis*, the preparation of which is very well known ; in this composition, the native acid of white tartar, or the chryftals of tartar, is impregnated with an alkaline ley, or *ol. tart. p. d.* from the union of these in due proportion, arises a mild neutral salt, which dissolves more easily in water, than tartar or its chryftals ; whence it hath the epithet of soluble. I put two ounces of this preparation into a small glass retort, and poured upon it one ounce of oil of vitriol,

vitriol, which raised a bubbling and effervescence. In distillation there came over six drachms of liquor, of a brownish colour, with a little oiliness upon it : its taste was sour, but disagreeable and empyreumatic ; it had an unpleasant smell, but not very strong or pungent ; it effervesced with alkaline salts fixt or volatile : in all which properties it agrees with the acid liquor drawn by distillation from crude tartar, which is commonly called its *spirit*. I observed, in distilling this mixture, that there came up some volatile salt, but in very small quantity, which spread itself upon the neck of the retort, in very fine and subtle ramifications : the upper part of the body of the retort was fullied with some thick black oil ; and the saline matter at the bottom was of a dirty ash colour, like tartar half-burnt. I poured warm water into the retort, and left it on hot sand, to dissolve the salt ; I found it necessary to pass this solution through a filter, and there remained in the paper a good deal of black earthy matter. The liquor was very acid, and had a vitriolic roughness, because more oil of vitriol had been poured upon the *tartarus solubilis* than was sufficient : to overcome the acidity, I dropt into the li-

quor some *ol. tart. p. d.* till the effervescence ceased ; and then, by evaporation and chry-
stallization, I procured a salt, in every respect,
like the *tartarus vitriolatus*, as usually pre-
pared.

UPON two ounces of the same *tart. solubil.*
I poured one ounce of spirit of sea salt, made
after *Glaubers* manner ; by distillation there
was produced a liquor of the same kind, with
that of the preceeding experiment. The re-
maining salt indeed was not dissolved nor
chrystallized ; but from its taste, and by its
precipitating a solution of silver in *aq. fort.*
it appeared to be of the muriatic kind.

AFTER this, I made an experiment on an-
other neutral salt in which the acid of vine-
gar is united to the alkaline salt of tartar,
which is commonly known by the name of
tartarus regeneratus. I took one ounce and
an half of salt of tartar, and poured distil-
led vinegar upon the dry salt at several times,
shaking the glass frequently, till fifteen oun-
ces of the acid were employed : the vessel
was then set on a sand-bath, and the liquor
gradually inspissated till there remained four
ounces 27 grains of a moist, greasy, saline
substance, of a brownish colour, like coarse
sugar.

sugar. I did not think it for my present purpose to bestow more time and pains to bring this salt to a greater degree of whiteness and dryness ; but I put it into a retort, and poured upon it one ounce and an half of oil of vitriol, diluted with three ounces of water : in distillation there came over near seven ounces of a thin limpid acid liquor, which, in taste, resembled distilled vinegar, but was more strong. The saline mass left in the retort being dissolved in warm water, the solution filtered and evaporated, the crystals could not be distinguished from the ordinary *tartarus vitriolatus* in shape, colour, or taste.

To satisfy myself, that a volatile alkaline salt or spirit may likewise be disengaged from an acid to which it has been united, and restored to its former strength & qualities, I made the following experiment. I took eight ounces of that compound saline liquor, which is described in our College dispensatory under the title of *Spiritus Mindereri* ; it is made by pouring distilled vinegar upon volatile sal-ammoniac, until the salt is dissolved and satiated by the acid. This neutral saline liquor, however, will not crystallize or coalesce

alesce into a dry solid salt, nor will the volatile *alkali* separate from the acid, by a simple distillation, but they rise together. I put eight ounces of this spirit into a bolt-head; and, adding half an ounce of dry salt of tartar, I presently fitted a capitel and receiver to the body, luted the joinings, and set the vessels in a sand heat; in a little time a white volatile salt sublimed into the capitel, which was soon washed down by a watery vapour into the receiver. This liquor being tried, was found equally strong, pungent, clear and volatile, as the best spirit of sal-ammoniac: the saline matter left in the body was of a greyish colour, soft and greasy to the touch, and would hardly dry or chrySTALLIZE, resembling *tartarus regeneratus*.

I need not confirm this by any further experiment, because it is known to every body, that the volatile salt and spirit of sal-ammoniac are procured by adding a fixt alkaline salt to the crude sal-ammoniac: now, as we can prepare an extemporaneous or artificial salt, by impregnating the volatile salt or spirit of sal-ammoniac with the spirit of sea salt; we have no reason to doubt, but, by means of

of a fixt alkaline salt, we may again disengage the volatile salt or spirit from the acid.

BUT it may be of more importance to discover what kind of acid is joined to the volatile alkali in the common or crude sal-ammoniac: for this purpose I made the following experiment. When all the volatile salt and spirit are drawn off from crude sal-ammoniac by the addition of a fixt alkaline salt and water, there remains a saline mass; which, when dissolved in warm water, the solution strained and evaporated, the chrystals very much resemble those of sea salt, in shape, taste, and other distinguishing characters of that salt, especially when it is well depurated and carefully chrystallized. This salt, however, has been dignified with the title of *sal febrifugum Sylvii*. I put three ounces of it into a retort, and poured in one ounce of oil of vitriol, and as much water. In the distillation, the receiver was filled with white fumes, which condensed into a liquor of a pale colour, somewhat turbid at first, of which I had near three ounces. The liquor had all the appearances of the spirit distilled in *Glauber's* manner, from common sea salt; and when some of it was dropt into the solutions

tions both of silver and quick-silver, it turned them both milky, and made a white precipitation. From all which I have no difficulty to conclude, that the acid contained in crude sal-ammoniac is of the muriatic kind, or the same which is produced from sea salt. But, whence this acid should proceed, is a matter of greater intricacy; since we are assured that no sea salt is necessary to, nor employed in, the preparation of that sal-ammoniac which is brought to us from *Egypt*.

THIS question I shall not undertake to discuss, as being foreign to my present purpose; but shall subjoin some remarks on the experiments above related, or deduce some corollaries which seem naturally to arise from them.

I. THE acid spirits, as those of vitriol, nitre, sea salt, vinegar, &c. are saline liquors, or fluid salts, made up of the very minute particles of an acid salt, and a certain proportion of phlegm or water.

II. THE particles of these acid salts cannot be brought to unite, among themselves, by chrySTALLIZATION or sublimation, so as to appear in the form of a solid or dry salt: but they

they readily lay hold on the particles of certain metals or semi-metals, calcarious or absorbent earths, or opposite salts, fixt or volatile, which serve them as a basis; and, joined to these, they assume a chrystalline form.

III. WHEN any of the acid liquors are joined, in due proportion, to any alkaline salt, whether fixt or volatile; the particular acrimony and pungency of both, as to our senses, and their power of dissolving certain bodies, in a separate state, are quite destroyed, at least, overcome and suspended, by their union.

IV. WHEN an acid liquor has dissolved a metallic body, and is united to it; the solution and the vitriol made of it, are frequently more acrid and corrosive, with respect to the human body, than the acid by itself. I do not affirm this universally: but we have instances of it in the solutions and vitriols of silver, quick-silver, copper, antimony.

V. WHEN the spirits of nitre or sea salt are joined to salt of tartar, or pot-ash, and chrystallized; the chrystals assume the peculiar shape, and most remarkable properties of the natural salt, from which the acid spirit

rit was originally produced. Consequently there appears to be some affinity betwixt the native fixt bases of these salts, and the fixt salts of plants.

VI. THE artificial vitriols compounded of the same acid spirits and metallic particles, do not resemble the original salts; but the vitriols of different metals, or metallic bodies, have different shapes, tho' they are joined to the same acid salts.

VII. THE acid salts, tho' combined with the alkaline, into mild neutral salts, yet are not so changed by this union but, when they are again artfully disjoined, they may exert the same powers and produce the same effects as before their union: therefore we may conclude, that the figure, bulk, and density of the acid particles are not changed tho' the liquors may differ somewhat in strength from the greater or smaller proportion of phlegm, or water accompanying the real acid.

VIII. ONE acid which has been united to an alkaline salt may be set at liberty, or expelled from its cohesion with the alkaline salt, by another acid of a different origin. But this does not happen indiscriminately; for

for the oil of vitriol expels any other acid, but cannot be dislodged by any of the rest. A volatile alkaline salt is expelled from its union with an acid, by a fixt alkaline, but not contraryways.

General Scholium.

WE can have no other idea of a solid or consistent body in general, as distinguished from a fluid, than that the parts of a solid cohere together by some power, which retains the particles in their situations, with respect to one another, and makes them to resist their separation or division, till that power is overcome by a superior force.

WHEN we observe that a certain fluid, as water, poured upon a hard solid body, as a lump of salt-petre, in some hours, makes such a change upon the lump, that it wholly disappears, if the glass is shaken a little, that is, the salt is divided into particles, so small, that they escape our sight, and are equally distributed through the liquor, and suspended in it; when we observe this, I say, we cannot but conclude, that the particles of

T t

water

water are endued with a power capable to overcome the force by which the particles of salt-petre cohered with one another.

IF a known quantity of cold water has dissolved as much salt-petre as it is capable to hold, in this solution the water is to the salt as $5 \frac{1}{3}$ to 1; (in scalding water the proportion is as 4 to 1). If then $\frac{4}{5}$ of the water is wasted by evaporation, many of the small particles of nitre being brought into contact, join together, and form chrystals in shape and other properties resembling the nitre which was dissolved in the water. It follows, therefore, that the particles of nitre mutually attract one another, when the interposing matter is removed, the distance betwixt them very much diminished, and no superior power acting upon them; and, when in contact, they cohere with a certain degree of force. Again, it appears, that the particles of water do attract the particles of nitre more strongly than these last attract one another; and, consequently, overcome their cohesion, and suspend their mutual attraction. Lastly, it is evident, from the above appearances, that tho' the parts of nitre which swim in the water are so small, that they
are

are not perceptible singly, even when viewed thro' a pretty good lens; yet they must be similar to one another, and retain the properties of real nitre.

LET us next consider the effects of another liquor, upon the same salt-petre.

IF oil of vitriol is poured upon a parcel of nitre, there happens likewise a solution, but with appearances different from those in the former; for this is accompanied with a considerable intestine motion and expansion, smoak and heat, and produces a much greater change on the nitre: for, by the help of external heat and close vessels, there rise from the mixture copious red steams, which are condensed into a corrosive acid liquor, very active and volatile, and there is left a white saline mass, no way resembling nitre.

FROM what has been said formerly of this experiment, it appears that the oil of vitriol makes a more minute and subtile division of nitre than what simple water can do; that the fluid acid salt of the oil of vitriol attracts one part of each real particle of nitre, *viz.* the fixt and alkaline basis, while this same vitriolic acid seems to repell another part of nitre, that is the acid and volatile
part,

part, which attracts, or is attracted by the phlegm or watry part of the mixture, is easily raised up in vapour by heat, and turned into a saline acid liquor.

It may be justly questioned, whether this is an ultimate division of nitre, or a separation of it into its most simple principles or first elements. It is very hard to find an experiment that can give full satisfaction in this point; for there are certain bounds set to art, as to the division of bodies: but if that part of nitre, which unites with the acid of vitriol into a neutral salt, is the same with nitre fixt by deflagrating with char-coal; or if it is no other than that salt which can be extracted from the remains of the distillation of nitre with bole; then we have some reason to conclude, that this part of nitre is not a simple elementary matter: since fixt nitre can, by frequent solutions and fusions, be so changed, that little of it will remain, but a mere earth.

MANY of the solutions which happen in chemistry, are of the first kind, that is, the solvents act no otherways upon their proper objects than water does upon nitre or any other salt, by dissolving the cohesion of the
larger

larger particles which compose the body, without resolving the particles into their primary elements.

WHEN spirit of nitre or *aqua fortis* is poured upon pieces of silver, the appearances plainly point out that there is an attraction betwixt the particles of the solvent and the silver, and that there is likewise a repulsion of the particles already dissolved from the silver, to make way for the application of new particles of the solvent to other parts of the silver; for there is a remarkable motion and agitation in the liquor, the surface of the silver soon becomes rough and discoloured, air bubbles are formed upon it, which soon fly off, and a constant succession of these arise, break and diffuse themselves: if a little of this liquor is carefully taken off from the top, even before half the silver is dissolved, and dropt into a *muria* of sea salt; it will give evident marks that it contains a portion of silver. When all the metal is dissolved, the solution becomes clear and limpid, and all is at rest. These things evidently shew, that such metallic particles as are freed from their cohesion by the saline parts of the *menstruum*, recede with them from the silver, and

and are succeeded by other acid particles to perform their part of the operation, until all the parts of the solvent are equally charged with particles of silver.

THE attractive force between the saline particles of the spirit of nitre and the silver, must be superior to the power by which the particles of silver attract one another ; for it dissolves their cohesion, separates them, and keeps them suspended in a fluid which has only about one tenth part of the specific gravity of silver : yet these particles, thus suspended, differ in nothing, when freed from the acid, but bulk from granulated silver or the bits rubbed off plate by a fine file. That this power of attraction between the particles of spirit of nitre and those of silver must be very great, will appear, if we consider, first, that it requires a very great heat to melt silver ; but this melting is nothing else, than, by the force of fire, to overcome the cohesion among the particles of silver, and bring the mass to a state of fluidity, so that the particles may easily move and slide one upon another while that degree of heat lasts ; but spirit of nitre produces the same or rather a greater effect. In the next place, tho' spirit

rit of nitre is of itself pretty volatile, yet, when it has dissolved silver, the saline parts adhere so firmly to the metallic, that a very strong degree of heat is required to separate them; for, if a solution of silver is set in a heat that would make water boil, the light and watry parts indeed fly off, but afterwards the evaporation goes on slowly, unless the fire is considerably increased; and, after several hours, the matter has still the appearance of a very moist salt, which is very corrosive, and, in bulk and weight, greatly exceeds the silver that was dissolved. When the sand-heat can make no further change on the mass, put it into a crucible, and set it in a pretty brisk and clear fire: this expells the remaining moisture and superfluous acid; after this the matter melts like wax, and, when cold, is turned to a hard substance; the caustic quality of which, the increase of weight above that of the silver, and its aptness to moisten, shew, that, after all the heat which the mass has sustained, there is still a good quantity of the saline part of the spirit of nitre intimately incorporated with the silver. Now, when a piece of this hard stony substance is put upon a piece of well kindled char-coal,

char-coal, and blown a little, it makes an explosion with a sparkling and flame, like a piece of common salt-petre; the saline matter is consumed or turned into flame or smoke, and the pure silver is left on the coal. By the way, this may be some sort of proof, that the volatile acid part of nitre is not more a simple elementary matter than is the fixt part.

NOTWITHSTANDING what has been shewn, concerning the strong attraction between the particles of spirit of nitre and those of silver, and the difficulty of separating them by the force of fire; yet, if a piece of clean copper is put into a solution of silver in spirit of nitre, the saline particles most readily shake off the silver, and dissolve the copper; which shews that the particles of copper have a stronger attractive power, with respect to the spirit of nitre, than the particles of silver have; which difference of attraction will probably arise from the different bulk, figure or density of the particles of the two metals, the acid being the same. The same thing may be said of iron, zinc, chalk, volatile alkaline salt and fixt salt, each of which attract spirit of nitre more strongly than

than silver does, and the last more than the preceeding.

HERE then we have a series of bodies from silver to salt of tartar, whose attractive powers, with respect to the same saline liquor, are continually increasing; but, whether uniformly or in any certain proportion, is not easy to determine: there may be found a body which attracts the acid of nitre more than the first, and less than the second, and so thro' other degrees; which can only be ascertained by a great variety of trials.

As the salt of tartar, or any strong fixt alkaline salt, is the strongest attracter of the nitrous acid, and throws down or separates any other body that has been dissolved in it; so it likewise attracts every other acid, and disjoins it from whatever it has dissolved: therefore we may consider *that* salt as a standard whereby to compare the several attractive powers of the different acid liquors. But it is certain, from the experiments that have been related, that, if three parcels of the same fixt alkaline salt, are joined to the three different acids of nitre, sea salt, and vinegar, and if oil of vitriol is poured upon each of these saline mixtures or compound salts; the vitri-

olic acid will attract, or be attracted more strongly by, the alkaline salt than the other acids, which will be driven from their places; and the acid of vitriol will be so firmly united to the fixt salt, that no other acid or known body is capable to separate them.

AGAIN, If spirit of nitre is poured upon a compound saline liquor, or salt made of spirit of sea-salt and salt of tartar, and upon another made of spirit of vinegar and the same salt of tartar; the spirit of nitre, by its superior attractive power, will join itself to the alkaline salt, and drive away the other acids; but is not able to resist the still greater force of the vitriolic acid.

LASTLY, If spirit of sea-salt is poured upon a compound of spirit of vinegar, or any other vegetable acid and a fixt alkaline salt; this will be more strongly attracted by the acid of sea-salt, than by the vegetable acid which will be expelled: but the spirit of sea-salt may likewise be dislodged by the acids of vitriol or nitre.

IN most solutions, precipitations, and other operations by which the texture of bodies are much changed, there are some appearances, such as an expansion and bubbling, the production

duction of an elastic fluid like air, and a commotion so great, as to produce some remarkable degree of heat, and in some cases smoak and flame: which appearances, I say, plainly discover, that the small particles of bodies are endowed with some very active powers, capable to introduce certain motions tending to union or disjunction, attraction or repulsion.

THAT many parts of matter are endowed with an attractive power tending to union and cohesion, may be deduced from the instances already mentioned: and many more might be brought from occurrences in chemistry to confirm the same. That other parts of matter are endowed with an elastic or repelling force, may be concluded from the properties of air and light, demonstrated by the greatest philosophers of the last and present age. The curious experiments of the ingenious Dr. *Hales*, in his *Analysis of the air*, and the surprising experiments on electricity now so much in vogue, may satisfy us, that particles of the same nature and qualities with those of air and light, may be joined to particles of a grosser nature and attractive quality, in various masses of matter, to some adhering more loosely, to others

thers more firmly connected. For some time they may lie concealed without manifest motion or action ; their repelling or elastic power being overcome or balanced by the attractive force of the surrounding corpuscles, till by certain concurring circumstances, as external motion, heat, moisture, the addition of other matter, &c. the attractive force is diminished, or the repelling increased : then they are roused, as it were, into action, and discover themselves by great and remarkable effects, in changing the texture of the masses to which they were united. Such changes we daily see happen in fermentations, putrefactions, effervescences, solutions, accensions, explosions, &c.

These principles of motion in matter, are not the vain fictions of men merely speculative in philosophy, but evidently deduced from observations and experiments on a great variety of bodies in many different circumstances ; and every one who reflects upon the most ordinary occurrences in the works of nature and art, will be convinced of the existence of such causes, and find that they are so universal and unalterable, that they can only be referred to the first cause, that is, to the will of the Supreme Being.

ART. XII.

Experiments and Observations upon the Hartfell Spaw, made at Moffat 1750 ; and an Account of its Medicinal Virtues, so far as they have hitherto been discovered from experience ; by WILLIAM HORSEBURGH, M. D.

THIS Spaw springs from the *Hartfell* mountain, about three miles north of *Moffat* ; and is commonly called *Williamson's water*, from Mr. *John Williamson*, who discovered it in 1748, and who has been at a good deal of pains, and some expence, to make it accessible, by mending the road, clearing away the dirt about the spring, and fitting it with a spout, that it may be more conveniently taken up.

The water on which I made the following experiments, had been taken up three or four days from the spring, and was contained in bottles well corked and waxed ; it was taken up on the 26th of *August*, after a very rainy season.

1. IT was quite clear and pellucid.

2. IT

2. It had a sharp, aluminous, strong chalybeate, and styptic taste.

3. It curdles with soap.

4. Its specific gravity to that of *Annan* river water (which is esteemed the best water they have hereabout) is, as four ounces five grains, to four ounces eleven grains.

5. By shaking it in a vial, or mixing it with tartish wine and sugar, it gives no signs of its containing a spirit more than river-water.

6. WHEN boiled with an equal quantity of new or sweet milk, it does not curdle the milk.

7. WITH syrup of violets, it gives a faint green.

8. WITH powder of galls, it presently became blue; and turned gradually darker, till it became of a deep purple, and then of an ink-colour: it does the like with an infusion of tea, tho' not in the same degree as with the galls.

9. WHEN *oleum vitrioli*, or *spiritus vitrioli*, is dropt into it, no effervescence, or change of colour, is produced.

10. WITH *oleum tartari per deliquium*, it made no effervescence; but light green clouds gathered,

gathered, and were suspended, in the middle of the water; and a great number of small air-bubbles, like particles of quick-silver, appeared adhering to the bottom of the glass, to its sides, and upon the surface of the water; multitudes of the like air-bubbles were intermixed with the green clouds: after standing about an hour, a great many of the air-bubbles disappeared, and the clouds in the middle of the water grew thicker, and of a darker green, and afterwards precipitated.

11. An aqueous solution of *saccharum Saturni*, turned it a little whitish, but not so much as it did the river-water; and much less than it did the river-water, when there was a very little sea salt dissolved in it; in all these three trials, a white powder precipitated.

12. ON *September 1st*, I poured a chopin or *English* quart of this water into a clean, white, stone-bowl; and covered it with a piece of paper, to keep out the dust.

ON the 2d, there was precipitated a brown powder, like brick-dust; and, on the surface of the water, there were many shining spangles of a copper-like colour.

ON

ON the 3d, there was more of the brownish powder at the bottom; all the parts of the bowl, which were covered with the water, were tinged yellowish; a pellicle covered the surface of the water, and all the spangles were gone.

ON the 6th, the sharp aluminous taste was pretty strong, but the chalybeate taste much weaker; there seemed to be no more of the brownish powder at the bottom than there was on the 3d; the yellow substance that adhered to the bowl was also thicker, and had a smooth oily look; the pellicle on the top had now acquired the thickness of writing paper; it was easily taken up, as it adhered to any thing put into it, and was of a golden colour: I took up as much of the water as filled a wine glass; and, with powder of galls, it immediately struck a blue.

ON the 13th, the sharp aluminous taste still remained strong, but the chalybeate was scarce to be distinguished; the quantity of the precipitate, and the pellicle on the top, were much the same; but the yellow substance, adhering to the bowl, was rather more: into a glass of this water, I dropt syrup of violets; it turned of a faint green: to another glass
of

of the same, I put powder of galls; it immediately struck a blue tincture, but not so deep as on the 6th.

ON *October* 9th, it had a more aluminous taste, than the fresh *Spaw* water; the chalybeate taste was scarce to be distinguished; the precipitate, yellow substance adhering to the bowl, and the pellicle on the surface, much the same as on the 13th *ult.*; the water was as limpid as on the first day; syrup of violets changed its colour to a deeper green than before, and, with powder of galls, it immediately gave a blue tincture.

13. I boiled a chopin of this *Spaw* water, in a clean tin pan, till there remained of it only one fourth part; it turned of a yellowish colour and was muddy: when this was cold, and the okry flakes precipitated, it had a harsher and more aluminous taste than the fresh *Spaw* water, or than that which had been so long exposed to the air (12.). To a glass of this I put powder of galls; it changed very slowly, and, after it had stood about eighteen hours, it exhibited a faint blue.

14. In a clean tin-pan, over the fire, I evaporated a chopin of the same *Spaw* water, till there remained only about one ounce and

a half; this I immediately poured into a China tea-cup, and, when cold, filtrated it: the filtrated liquor was clear and limpid, and had a strong chalybeate taste; this was evaporated by a sand-heat, in a wide-mouthed glass; there was left a salt of a whitish-brown colour, which had an aluminous, and strong chalybeate rough taste. The whitish-brown colour of the salt was owing, I believe, to its having been a little scorched at the bottom of the glass; for, when I afterwards gently exhaled the water before the fire, the salt was of a pure white.

15. A little of this salt put into a glass of water, made the water of a yellowish colour; and, in a little time, it began to separate into small yellowish coloured flakes, which growing gradually larger, and thicker, precipitated; after standing a night, the water was quite clear, with a few detached, small flakes, floating close to the sides of the glass; the water had the original taste of the *Spaw*, but rather stronger.

I poured off the clear water, half into one glass, and half into another: into the first poured off, which was the clearest, I dropt syrup of violets; after standing a little,
it

it gave a faint green tincture : to the other half, I added powder of galls ; it slowly changed to a violet colour, then to a purple, and, after standing all night, it was of a very dark blue, inclining to black : into the remaining water, with the sediment, I dropt syrup of violets, and stirred all about ; it caused a tincture of a deeper green than the clear water : all these three mixtures, which did not exceed an ounce and a half, I poured into a chopin of common water ; and the whole became of a deep violet colour.

16. THIS salt, after *ol. tartari p. d.* has been dropt upon it, and then dissolved in a little water, yields a deep green.

17. THE salt of this *Sparw*, after it is burnt on a hot iron, exhibites the like *phænomena* (15), and always loses its aluminous taste.

18. THIS salt turned moist and bubbled on a hot iron ; and, when calcined in a crucible, it became of a purple colour, exactly like that of the *vitrioli Martis calx rubra* ; and there was a manifest attraction of several of its particles by the magnet.

19. NEITHER the salt nor earth of this *Sparw*, make any effervescence with *spiritus vitrioli*,

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vitrioli, *oleum vitrioli*, or *oleum tartari*, *p. d.* when by themselves, or when diluted with water ; nor does the salt emit any fumes with *ol. vitrioli*.

20. A glass of this water, into which ten gutts of *ol. tart. p. d.* were dropt, and another glass of the same water, turned, with powder of galls, to a faint violet colour ; being mixed together, produced exactly the colour of old red port-wine.

Experiments made on the same Spaw Water, taken up October 12. 1750, after five or six Weeks of dry Weather.

21. The sharp aluminous taste was much weaker, and the chalybeate stiptic taste scarce to be distinguished.

22. WITH syrup of violets, even after standing all night, there was a little, and but very little, tendency to green.

23. WITH *ol. vitrioli*, no effervescence, or change of colour.

24. WITH *ol. tart. p. d.* no effervescence ; but the like clouds appeared as (10), tho' in smaller quantity, and of a yellow colour, with scarce any green.

25. WITH

25. WITH the solution of *saccharum Saturni*, it gave a little milkiness, but scarce to be distinguished.

26. WITH powder of galls, it changed very slowly; and, after standing a night, it exhibited but a faint violet colour, something inclining to purple.

27. A chopin of it, evaporated in a clean tin-pan, yielded a very white saline substance, of a sharp, aluminous, chalybeate and stip-tic taste.

28. A chopin of this *Spaw* water, taken up from the spring, after a long rainy-season, yielded of salt, nine grains; and of earth, something more than one grain: the like quantity taken up after three weeks of dry weather, gave of salt, but seven grains; and of earth, about half a grain: the like quantity taken up, after between five and six weeks of dry weather, afforded of salt, only five grains and a half; and of earth, about a quarter of a grain.

29. THERE is another spring, about forty foot from this, which seems to be of the same kind; for it has the like taste; and, with syrup of violets, powder of galls, *ol. vitrioli*, *ol. tartari*, *p. d.* and the solution
of

saccharum Saturni, it exhibites the like *phænomena*.

From the preceeding Experiments we may observe,

1. THAT this *Spaw* contains a principle of iron, which is volatile ; for its chalybeate quality is much weakened by boiling (13), or even by being exposed for some time to the air (12).

2. IT contains an iron-principle, which is fixed ; for its chalybeate quality is not intirely destroyed, by being exposed to the air (12), nor by boiling (13), nor by evaporation to a dryness (14, 15), nor by burning the *residuum* (17) ; and, when its salt is calcined a sufficient time, it is attracted by the magnet (18).

3. IT seems, by turning green with syrup of violets, to contain an alkaline principle, which is likeways fixed ; for neither by being exposed to the air, nor by evaporation to a dryness, nor by burning the *residuum*, does it lose this quality.

As neither the water nor its salt or earth, make the least visible effervescence with *spiritus*, or *ol. vitrioli*, either by themselves, or
when

when they are diluted with water ; so, it may be said, that it contains no alcali, and that its turning green with syrup of violets, is of itself no proof ; because common water does, with syrup of violets, change to a faint green colour, after it has stood sometime : but this I imagine rather to be a proof, that there is in common water an alkaline principle, viz. absorbent earth ; for rain-water, or snow-water, have not the least tendency to green, with the said syrup.

4. IT seems to contain a very little sea salt (less than common water) by its precipitating the solution of *saccharum Saturni* (11).

5. IT seems to contain some portion of *alum.*

α. FROM the taste of the *Spaw* water, as it comes from the spring, which is remarkably aluminous.

6. FROM its taste becoming more aluminous, when part of the water is exhaled, by being exposed to the air, for between five and six weeks (12).

γ. FROM its taste after three fourths of the water was evaporated, which was more harsh and aluminous, than that of the fresh *Spaw*,
or

or than that which had been so long exposed to the air (13).

δ. FROM the aluminous taste of the salt (14, 27).

ε. FROM its salt swelling and rising into a blister on the hot iron (18).

ζ. FROM this salt losing its aluminous taste when calcined a sufficient time (17), which also happens to *alum*, when treated in a like manner.

THE *nitrum murale*, it is true, also swells and rises in blisters on the hot iron; likewise *borax*, and the purging salt of *Scarborough-water*: but the *nitrum murale* differs from this salt in taste, and, when calcined, falls down into a loose powder, like lime; which this salt did not do, tho' calcined for a considerable time, in a strong fire.

Borax likewise differs in taste from this salt, and vitrifies in a moderate heat, in a few minutes; which this salt did not, after being calcined in a strong heat for a considerable time. This salt has not the bitter penetrating taste of the purging *Scarborough-salt*, neither its purging quality.

I don't however pretend that all those marks amount to a demonstrative proof that
there

there is *alum* in this water, but only to a great probability: for, to be sure of it, we must see the *alum* chrystallised, which is not easily done *; and requires, for that process, a greater quantity of salt than I had.

It may be objected, that, when equal quantities of this water and new milk were boiled together, the milk did not curdle(6.); therefore there can be no *alum* in it. This objection is specious; its validity, however, like that of many other things which have very plausible appearances, can only be determined from experiment: wherefore I dissolved in water a greater proportion of *alum*, than there is, or seems to be, in this *Spaw*; and, when equal quantities of this solution, and fresh milk, were boiled together, the milk did not curdle.

6. THIS water contains very little earth, it gives not the least sign of it, when *ol. tart. p. d.* is dropt into it; but the little it contains, seems to be of the white calcarious kind: and tho' okry-earth is commonly allowed to be a constituent principle in chalybeate waters,
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* Vid. SHAW's Inquiry into Scarborough-water, part 1. § 4. art. 43.

ters, yet I am of opinion, that no such thing exists in them in a sound, natural state ; and that it is never to be found, till the acid, or whatever it is, that dissolves the iron-principle, leaves the water, or, at least, its union with the iron-principle, then it appears in the form of an okry substance ; but, while the dissolvent of the chalybeate principle remains united to it, that is, as long as the water continues in a sound state, okry-earth is never to be found in it. The earth, therefore, of this kind, which is left in the filter after evaporation and filtration, is owing to its dissolvent principle being lost in that process. And I have reason to believe, from experiments which I have made, that, by repeated solutions, evaporations, and filtrations, the purest and most genuine crystals of *vitriolum Martis* may be all reduced to an okry, or bolar earth ; especially if the solutions be made in common water.

7. THE quantity of mineral principles in this water varies ; and is less after a dry season, than a wet one (28) ; an unusual circumstance, which probably proceeds from this cause, that the water in a wet season, rises higher in the veins, which contain the mineral

ral principles, and consequently dissolves and carries a greater quantity of these principles along with it. Whence it is likewise evident, that experiments made on this water at different times, will not always exactly correspond, but will vary more or less, in proportion to the mineral contents of the water.

8. THE tenth experiment seems to be an indication, that this water contains a *vitriolum Martis nativum*; no other mineral water, that I know of, turns green, with *ol. tart. p. d.*: but, when this oil is dropt into a solution of *vitriolum Martis*, it becomes green. And tho' there is no visible effervescence in this experiment (10.), yet the many air-bubbles generated seems to be the effect of the alkali's uniting itself to the acid, which kept the chalybeate principle dissolved, and which chalybeate principle, being now by the *ol. tart.* disengaged from its dissolvent, appears in the form of green or yellow clouds; and at last is found, at the bottom of the glass, under the form of a subtile okry, or bolar earth.

It may perhaps be said, that since there is probably *alum* in this water, the air-bubbles are generated by the alkali's uniting itself to the acid of the alum. To destroy this supposition,

position, I dissolved *alum* in water, and that in a greater proportion, than it can be in this *Spaw*, and then dropt into it *ol. tart. p. d.*; there ensued no effervescence, or sensible change upon the water; but a few air-bubbles gathered at the bottom of the glass; nothing, however, in comparison of what appeared when the *ol. tartar. p. d.* was dropt into the *Spaw*; and no more than what formed upon dropping *ol. tart. p. d.* into a glass of pure river-water.

9. As chalybeate waters, in several particulars, resemble a solution of *vitriolum Martis* in water, hence many have been induced to think, that those waters contained a *vitriolum Martis* such as is prepared by art: but some of the latest, and most learned authors, as *Hoffman, Boerhaave, &c.* are of a contrary opinion, and for a very good reason, because no *vitriolum Martis* could ever be found, upon evaporation of chalybeate waters: the experiments however made on this *Spaw*, seem to prove that it contains a fixed vitriol of iron; for it not only turns green, upon the addition of *ol. tart. (10.)*, but likewise, upon evaporation, there is a salt left, which has all the properties of *vitriolum Martis*.

I dissolved, in a chopin of fountain water, *vitriol. Mart. gr. xv.* and in every respect treated it as I had done the *Hartfell Spaw-water*, viz. it was evaporated over the fire, till there remained about two ounces; this was filtrated, and exhaled, in a wide glass, before the fire: there was left in the glass *gr. xij*s of a light-yellowish coloured salt, and in the filter a dark-brown earth. With this salt, I repeated all the experiments I had made with the salt of the *Hartfell Spaw*; and I had the pleasure to see, that they corresponded in every thing, without any material difference; only the salt left upon evaporation of *vitriolum Martis*, did not rise in a blister on the hot iron, and, when dissolved in water, gave, with powder of galls, a redish tincture inclining to purple: whereas the salt of the *Hartfell Spaw*, being dissolved in water, with galls, struck a blue. The reason of which I take to be, that there is a greater proportion of acid, in the composition of the salt of the *Hartfell Spaw*, than there is in the *vitriolum Martis*; for, if you add an alkali, as *ol. tartari*, to the blue tincture, produced by the salt of *Hartfell Spaw*, it changes to a redish; and if an acid, as *ol. vitrioli*, is added

to the *vitriolum Martis*, (e. g. if to a solution of two, three, or four grains of *vitriolum Martis*, in eight ounces of water, be dropt one gutt. of *ol. vitrioli*) this, with powder of galls, tho' indeed it changes but slowly, will give a blue, not a redish tincture; and again, if the acid is weakened, by dropping into this blue tincture *ol. tartari*, the blue will change to a redish. And this conjecture seems to be confirmed by the following experiment, *viz.* if *vitriol. Mart.* be burnt on a hot iron, till it becomes quite dry, and of a yellowish brown, and dissolved in water, it gives much the same redish purple with galls, as before it was burnt; if this burnt *sal Martis* be powdered, and the dry powder wrapt up in a single paper, and left in a room without a fire for three or four weeks, it will become moist, and, when dissolved in water, strikes with galls no more the former colour, but a blue; which, I presume, is owing to the *acidum vagum* which it has imbibed.

SEEING therefore the salt of *Hartfell Sparo* has all the properties of *vitriolum Martis*, I cannot see, why it may not be allowed, that this *Sparo* contains a native, fixed vitriol of iron: and it is the only water (I know of)

which

which has been discovered to contain such a salt.

IN the Philosophical Transactions, N^o 245. mention is made of two mineral waters, which yielded a real, fixed vitriol of iron, one near *Eglingham* in *Northumberland*, the other near *Haigh* in *Lancashire*; but these waters were found by Dr. *Cay*, who made particular inquiry into the matter, not to be mineral springs, but drifts made for the draining of coal-pits; and that the vitriol found in these waters, was owing to their running over marcasite beds which had been exposed for some time to the air. I was suspicious that something of this kind had communicated the vitriol to the *Hartfell Spaw*; but Mr. *Williamson*, who discovered the spring, assured me, that the water, on which I made the preceding experiments, had run over nothing at all, after its exit from the spring, but was taken up by himself immediately, as it issued out of the rock. Dr. *James Hunter*, Physician in *Moffat*, afterwards sent me some of the same water, which he received into bottles, by applying their mouth to the orifice of the vein, in the rock from which it issued; and from it I obtained crystals of *vitriolum*

olum Martis, which were of a paler green than the artificial.

IN *August* 1752, this *Sparw*, with syrup of violets, struck no green tincture.

WITH *ol. tart. p. d.* it immediately turned green, and clouds of a deeper green and more in quantity than in *August* 1750, were formed, and that much sooner than in the preceding experiments.

WITH powder of galls it scarce changed its colour; and, even after standing some time, it exhibited but a faint blue. I was at a loss for the reason of its giving so faint a blue with galls; when, at the same time, I knew, by its immediately turning green with the *ol. tart.*, together with the quantity of the clouds that presently formed, and the deepness of their green, that it was very strong of the chalybeate principle: I therefore set the mixture before the fire, to see what change a little heat would produce; and, when I observed that a small heat had no effect, I gradually increased the heat, till bubbles appeared on the surface of the water; but this occasioned no change on the colour of the water, the faint blue remaining much the same. I then conjectured, from the wa-
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ter not turning green with syrup of violets, as in the former experiments, that there was a greater quantity of acid than usual united to the chalybeate principle: and, to discover whether my suspicion was right, I dropt into the same glass, containing the water and powder of galls, one or two gutts (I don't remember which) of *ol. tart.*; and, having mixed it by stirring, it immediately produced very deep blue.

FROM this experiment, I think it appears, that a water may be very strong of the chalybeate principle, and yet discover very little of it by the common experiment with galls; so that it may be proper always to try it with the *ol. tart.* also: for the strength of the iron-principle is, as the colour and quantity of the clouds formed by this oil; that is, if the clouds are of a light yellowish colour and small in quantity, the chalybeate principle is weaker; if they are of dark brown and more in quantity, it is stronger; if they are of a green colour, the deeper the green and more in quantity, the stronger is the chalybeate principle; which I have found to correspond with truth, by repeated experiments, not only on this *Spaw*, but on solutions of *vitriolum*

Martis of different strengths, in common water : but, how far this will hold in chalybeate waters that are volatile, I have had no opportunity to try.

I took a chopin bottle of this water, three months after it had been carried fifty six miles, and poured one half of it into another clean chopin bottle, and then corked them both very tight ; the one bottle I shook frequently and strongly for two days ; the third day, I tried both, with powder of galls and *ol. tart. p. d.* ; with the galls, both gave a very deep blue ; but the water which had been shaken, with the *ol. tart.* rather yielded a fainter green, than that which had not : I let both bottles stand corked up as before for ten or twelve days longer, without shaking ; then tried them again with the galls and *ol. tart.* ; they both struck much the same deep blue, tho' not so deep as at first ; and, with *ol. tart.* the water which had not been shaken, gave a green, tho' fainter than at first ; the other scarce offered any green ; the clouds formed in it were fewer, and of a lighter yellow, than those of the other. From which it seems, that shaking somewhat weakens the
chalybeate

chalybeate principle of this water, tho' not much nor suddenly.

THIS water seems to belong to the class of the *aqua Martiatæ*; for it effervesces not with acids, like the *acidulæ* and *thermæ*; neither is its volatile iron-principle so volatile as theirs: hence in general, it may be said to be aperient and strengthening, both when used internally and externally: whence it must be of use in diseases where the solids are relaxed and the blood too watery and weak. But, altho', when the principles of a mineral water are known, we may, from analogy, deduce its virtues in particular diseases; yet, as this method is not so certain, no reasoning being equal to experience, I shall confine myself to the last alone; tho' it is to be presumed, that there have not been yet sufficient opportunities of discovering all the virtues of a water so lately found out. However, it has been observed to be of great use in curing itchy, hot, tettrarious eruptions, old obstinate ulcers and sores, internally used, and externally applied: it has likewise been of great service in disorders of the stomach and bowels; in the bloody flux; bloody urine; spitting of blood; immoderate flux
of

of the menses; obstruction of the menses; the fluor *albus*; gleet; rheumatic pains; in the first stage of consumptions, and even when they have been further advanced; in preventing miscarriages; and in restoring health, when the constitution has been impaired by long illness: all which will appear from the following histories, most of which were wrote down from the patients own mouths, and attested by the physician or surgeon who attended them*.

BEFORE

* There were given in to the Society, with this paper, twenty two well attested histories of patients cured of the above mentioned diseases, by means of the *Hartfell Spaw*; which it was thought proper to omit, since they would have swelled this article to a bulk too great for a work of this kind. But as the good effects of this water in consumptions of the lungs are somewhat extraordinary, and may, perhaps, be doubted of by those, who have imbibed early prejudices against all chalybeates and astringents, in such cases; it may not be amiss to give a place, here, to the two following histories.

1. Mrs Glendinning, wife to Mr. Robert Glendinning, school-master in *Moffat*, aged 51, was, on the 20th of December 1750, attacked with pains in her right side; a constant sharp pricking pain under the middle of the breast-bone; a hard, tickling, dry cough; thirst, difficulty of breathing, and frequent gripes in her guts: all which symptoms increased, till the first of January 1751, when she began to spit, with difficulty, a little thick gross matter. In the beginning of February, the spitting was so extremely fetid and ill-tasted, that

BEFORE one enters upon a course of the water, it will be proper to cleanse the *primæ viæ*, by an emetic and a gentle purge or two; unless forbid by some particular circumstances of

that it made her often vomit: she frequently washed her mouth with salt and water; but still felt a taste and smell which she thought resembled that of stinking flesh: she now became very weak and emaciated; had hectic fits and night-sweats: which symptoms continued increasing till the middle of *April*; about which time (having tried no medicines before) she began to drink the *Hartfell Sparw*, to the quantity of half an *Englisch* pint every morning. She had scarcely used it a week, when her breathing became much easier: after a fortnight, the spitting lost the foetid smell and ill-taste; and, in three weeks, she was perfectly free of the pain of her breast, cough, spitting, difficulty of breathing, hectic fits and morning-sweats: and has continued ever since in good health.

2. Mrs. *Halliday* in *Barntympan*, aged 28, of a plethoric habit, complained, in the beginning of *September* 1750, of a pain under the upper part of her breast-bone, which frequently darted to the point of her right-shoulder; a pain in her head and neck; her breathing was not quite free, and she could not lie upon her right-side. About the middle of *October*, she began to spit florid blood, which continued three weeks, at the rate of a spoon-ful every day; then stopped a week, and returned again. In this manner it went on about four months: after which, beginning to fear the consequence might be fatal, as many of her nearest relations had been carried off by consumptions of the lungs; she applied to Mr. *Johnson* surgeon-apothecary in *Moffat*, who, finding the spitting of blood increasing, and her pulse pretty

of the patient : or, if the patient's body has been long costive, and the excrements hardened, emollient glysters may be used. Such

as

pretty full and frequent, ordered her to be bled, to take the *decoctum tamarindorum* of the Edinburgh Dispensatory, and to swallow four of the following pills thrice a-day. *R Extract. cort. Peruvian. drach. ii. balsam. Peruvian. scrup. i. pulv. cort. Peruvian. q. s. ut f. massa, ex cujus singul. drachm. formetur pil. xii.* The bleeding and decoction were repeated as often as they seemed necessary; and the pills were continued ten weeks without success: for soon, after she left off using them, she began to cough up purulent matter with blood. Whenever the spitting diminished, her breathing became very difficult; and the pain of her breast increased, with a particular foreness all along the breast-bone when she coughed.

She drank goat's and ewe's whey, from the beginning of June, to the end of August, without any sensible benefit; for she continued still to spit blood and matter: soon after this, she was attacked with sudden flushes of heat and morning sweats, which increased till the middle of January 1752, when she was become feebler, much emaciated, and often faintish. At this time, she was advised to drink the *Hartfell Sparw* in small quantity; but, finding it sit easy on her stomach, she drunk an *English* pint of it daily. During the first week after drinking the water, she expectorated dark, blueish, putrid stuff, without blood; and found her breast much easier than it had been any time from the beginning of her illness. Soon after, she began to breathe freely; and, after having drank the water six weeks, was perfectly recovered: since which time, she walked, or rather run, three *Scotch* miles in an hour, without being either hurt, or much fatigued by it.

as are plethoric, or have their vessels full, should let a little blood.

THIS water may be drunk at all times of the year : tho' I prefer the summer or warm months ; because perspiration is then freest, and the operation of the water, as an alterative, is assisted by the warmth of the weather : besides, this season is more convenient for the use of exercise, and the patients are less apt to catch cold. The properest time of the day for drinking it, is, when the stomach is most empty, as in the morning, fasting, before dinner, and sometimes in the evening. Some patients have a custom of drinking it with their victuals : but I don't approve of this ; as it may retard, or, in some measure, hurt digestion.

THE quantity to be drunk is to be determined by the age, strength, and other circumstances of the patient : they ought to begin with a small dose at first, and increase it gradually : such as are very weak and much extenuated, should begin with a gill or less ; this quantity they may take twice a-day ; and, by degrees, increase it, in proportion as they recover their strength, till they come to drink an *English* pint a-day, a pint and an half,

half, or two pints, which is about a *Scotch* chopin, and is the largest quantity usually drunk of this water in twenty four hours. If the water should be uneasy on the stomach, the patient may mix with it an equal quantity, or less, of new milk, or such a proportion of it as may be found most convenient: it may also be proper to warm the water.

As few of the patients, whose cases I have collected, observed any rule in their diet or exercise, and nevertheless met with a cure; this shows that there is no great strictness required in these matters: and, indeed, such as have any tolerable degree of health and strength, need not much recede from their ordinary way of living; tho' temperance in eating, a moderate use of wine, and gentle exercise, is what all infirm people should conform themselves to: but, such as are very weak, and much emaciated, or that labour under dangerous diseases of the lungs, or abdominal *viscera*, must observe a very strict regimen; their diet should consist of what is lightest and easiest of digestion, such as new milk, broths, panadoes, thin jellies, fresh eggs, chicken, veal, fowls, &c.; and a little wine may be allowed, when not prohibited

bited by the circumstances of the patient; moderate exercise may be proper for some, and rest for others; they must avoid the night air, sitting on the grass, and every thing that discomposes the mind.

UPON the first use of this water, some complain that it makes them sick, others that it makes their head giddy; but these inconveniences almost always proceed from their having taken too large a quantity: the dose is therefore to be diminished; and if, in a smaller quantity, it should be uneasy on the stomach, chewing cinnamon, or caraway seeds, or comfits made of them, are frequently useful; or any other aromatic, most agreeable to the patient. It sometimes occasions gripes, when the intestines are weak, on account of its coldness; it ought therefore to be warmed; the best way of doing it, is, to put as much as the patient intends to drink at one time, into a vial; cork the vial, and set it before the fire, or put it into warm water, till it acquires a sufficient degree of heat. On its first use, it sometimes purges such as are troubled with a *diarrhæa*; but that is to be restrained by a gentle emetic, or by giving some gutts of the *tinct. opii* in every

A a a

dose

dose of the water, or rather some little time before it; or three or four grains of the *pil. Matthæi* may be given at bed time, and as much, if requisite, in the morning: it will be convenient likewise to warm the water. But it oftener occasions costiveness; for which it will be proper, now and then, to give a little *sal. Glaub. polychr. manna*, or cream of tartar; or, which perhaps is preferable to any of them, the purging salt of *Scarborough-water*. If, upon catching cold, any feverish fits should arise, the use of the water must be suspended till these symptoms are gone. When the water does not pass easily, it will be proper to loosen the belly, or give diuretics; and such whose strength will permit, and whose vessels are full of blood and juices, should let blood.

THERE is no determining precisely how long it should be drunk; the advantage the patient reaps from it must decide that: some have used it only twelve days, others three weeks, others four, others six, some two months, and others many months; and yet they have all been so happy as to meet with a cure: so that, in general, as long as there is any prospect of its working a cure,

its

its use should be persisted in; and, tho' the patient may think himself well, I am of opinion he should not immediately discontinue its use, but gradually diminish the quantity, and leave off by degrees. Some recommend purging, and other medicines, after the course is finished; but, if the patient has been cured, all medicines are useless, and rather hurtful; if the cure has been imperfect, then such helps may be called in as the case requires.

As to the external use of this water: the practice has been, to wash with it, when warmed, itchy, hot, tetterous eruptions, and old obstinate ulcers, two or three times a-day, and, in some cases, during the whole day; to keep linnen cloaths wetted in the same water, applied to the parts affected; which method has been very successful: but, it must be remarked, that the water was always used internally also, in the same cases.

It seems proper to observe, that, as this water, put in bottles, well corked and waxed, may be carried to any distance, and will keep good a long time; longer than either the *Pymont* water or *Liege Spaw*: people living in any part of *Great Britain*, or even beyond seas, may reap almost the same benefit from it, as those who reside in its neighbourhood.

A R T.

ART. XIII.

*Belgic in
Lithography
Verhandelt.*

4. Dec 4. p. 22 Of the various Strength of different Lime-waters, by ROBERT WHYTT, M. D. F. R. S. Fellow of the Royal College of Physicians, and Professor of Medicine in the University of Edinburgh.

THE Reverend and ingenious Dr. Stephen Hales, having informed me, in a letter dated May 1751, that he had found the strength of lime-water much increased, by pouring it a second time on quick-lime, fresh from the fire; I thought it might be worth while to make a few experiments, in order to determine, with some degree of certainty, the different strength of different lime-waters: from these experiments it appeared, that lime-water acquired a considerable addition of strength by being poured on quick-lime newly taken from the fire; and that the first water got off quick-lime, was sensibly stronger than the fourth and succeeding ones *.

ON

* Essay on the virtues of lime-water, &c. p. 38 & 39.

ON the other hand, my worthy friend and Colleague Dr. *Alston*, having observed, several years since, that quick-lime continued to communicate its virtues to water much longer, than any one, before, had imagined; tells us, that he found afterwards, by experiments, that half a dram of stone quick-lime yielded forty ounces of lime-water; and that, after a pound of the same quick-lime had afforded five hundred pounds of lime-water, the water procured from it was as strong of the lime as ever *. Hence he imagines, that as water can only be impregnated, to a certain degree, by quick-lime, so this will happen equally, whether the quick-lime be fresh from the fire, or has had five hundred times its weight of water poured on it before; provided the water be allowed time enough to extract the virtues of the lime †. And further affirms, that the strength of lime-water cannot be increased by slaking new-made lime in it, because the water can take up no more of the lime than it had before ‡.

As

* Philosoph. Transact. vol. 47. p. 266, and Dissertation on quick-lime, &c. p. 4, 5, & 6. where the same thing is affirmed of oyster-shell-lime.

† Dissert. on quick-lime, p. 11. & 53.

‡ Id. p. 11.

As these experiments and conclusions appeared inconsistent with what I had advanced, the *Doctor* has endeavoured further to weaken the credit of my experiments, by some arguments drawn, chiefly, from the imperfection of the hydrostatical balance, and from the nature of quick-lime and its water. In order therefore to know whether I might not have been mistaken in what I had said, concerning the strength of different lime-waters, I thought it necessary to make some new experiments; an account of which I beg leave to lay before the society.

I. (a) HAVING got from my ingenious friend Mr. *James Gray*, a cylindrical copper-vessel ending in a narrow neck, which contained exactly 100 cubical inches; I filled it with the fountain water of this city, and, by means of a very nice balance, found it weighed 25320 *Troy* grains *, besides the weight of the vessel itself, which amounted to 13055 grains. (b) I

* According to Mr. *Gray's* experiments, the water which this vessel contains, only weighs 25318 grains, *i. e.* two grains less than we have made it. See above, p. 201. This difference may have arisen from our having put a few more drops of water into the vessel than Mr. *Gray* did. But altho'

(b) I poured upon 90 grains of calcined oyster-shells, newly taken from the fire, and reduced to a powder, 96 ounces, or five hundred and twelve times their weight of boiling water. After 92 hours, during which time the infusion was frequently stirred and shaken, I decanted off the clear water, and filtered it thro' a piece of very thick linnen-cloth doubled; by which means it was rendered free of any crusts, and equally pellucid with fountain water. With this lime-water I filled the above vessel, and found its weight to be 25356 grains*.

(c) MONDAY, at seven in the evening, I poured upon a pound of calcined oyster-shells, fresh from the fire, ten times their weight of water; next morning at ten, I decanted off the clear lime-water; and having filtered it

altho, in weighing fluids with this vessel, one might err six times more than this, yet it would not affect the point we have in view, which is not to determine with the greatest accuracy the different specific gravities of different lime-waters, but *only* to shew that they *are* different.

* The oyster-shells made use of in this, and the following experiments, were got from among the rubbish on the south side of the Castle of *Edinburgh*, and were quite free of any sea salt.

it as above, filled the *vessel* with it ; it weighed 25397 grains.

(*d*) TUESDAY at mid-day, I poured seven pounds of the single lime-water, (*c*) upon one pound of calcined oistershells, newly taken from the fire, stirring them well for some time after ; at three quarters past six in the evening, I decanted off, and filtered as above, the clear lime water ; and, having filled the *vessel* with it, found its weight to be 25457 grains.

HENCE it appears, that 100 cubical inches of the lime-water (*b*) exceeds, in weight, that quantity of fountain water by 36 grains : (*c*) exceeds it by 77 grains, and (*d*) by 137 grains.

THE specific gravity therefore of the weak lime-water (*b*) is to that of fountain-water nearly, as 704 to 703. The specific gravity of the single lime-water (*c*) is to that of common water, nearly as 329 to 328 ; and the double lime-water (*d*) is in specific gravity to water nearly, as 186 to 185.

IT is observable that the specific gravities of the single and double lime-waters (*c*) and (*d*), are considerably less than the specific gravities of the single and double lime-waters (*a*)
and

A. and B. mentioned *p.* 39. and 40. of my Essay on the virtues of lime-water, &c. But, if it be considered, that, in making the latter, a much less proportion of water was added to the quick-lime, than in making the former; it will appear that this difference of their specific gravities, does not infer any thing against the accuracy of the hydrostatic balance; but clearly shews, that the strength of lime-water varies according to the quantity of water poured on the quick-lime.

It may be worth while to observe, that the specific gravities of the lime-waters, (*b*) (*c*) and (*d*) did not differ more than their tastes. The first was weakest and least disagreeable; the second was stronger; and the third still stronger and somewhat pungent. Further, while the double lime-water (*d*) gave, in a few minutes, a copper-colour to silver; the weak lime-water (*b*) produced no sensible change upon it.

II. HAVING, formerly, found that lime-water and claret wine, mixed together, in a certain proportion, acquired a colour like that of gun-powder *: I thought, that, by mixing claret with different lime-waters, one

B b b

might

might judge whether they were all equally strong of the lime or not. The result of the experiments was, that one tea-spoonful of claret required four tea-spoonfuls of the lime-water (*b*); two and about one third of (*c*); and one and a half of (*d*), to give it the full gun-powder colour. These experiments, tho' not so accurate as those made with the balance, yet clearly demonstrate a remarkable difference of strength betwixt the above lime-waters.

III. TWENTY grains of salt of tartar being mixed with eight ounces and two drams of the weak lime-water (*b*), after it had stood five days on the lime, the mixture became immediately white and turbid, and soon precipitated a white powder; which, being separated from the water, by filtration, and dried, weighed $2\frac{2}{3}$ gr.

THE same quantity of salt of tartar, mixed with eight ounces and two drams of double lime-water, that had stood eight days on the lime, became considerably thicker and whiter than the former; and afforded rather more than 7 grains of white powder.

THE same quantity of salt of tartar being mixed with eight ounces and two drams of the

the double lime-water (*d*), which had stood 24 hours on the lime, gave eight grains of a white powder.

It was observable, that these three lime-waters retained the taste of the lime, after being mixed with the salt of tartar, and this equally after precipitation, as before it.

SINCE the earthy powder precipitated by these different lime-waters, proceeds *wholly*, or *almost* wholly, from the waters, and not from the fixed alkaline salt; * these experiments shew, beyond doubt, that double lime-water may contain thrice as much lime, as lime-water made by pouring, on quick-lime, 512 times its weight of water.

IV. 1. MONDAY 24th *December*, at eight in the evening, I poured upon a dram of fresh calcined oyster-shells, reduced to a powder, 520 drams of boiling water.

2. AT

* What proves this, is, that the calcarious matter precipitated by mixing salt of tartar with lime-water, is greater or less, in proportion to the strength and quantity of the lime-water; but not in proportion to the quantity of the salt. Thus 12 grains of salt of tartar, mixed with four ounces of strong lime-water, yielded as much of this matter, as the same quantity of this lime-water, mixed with 18 grains of the salt.

2. AT the same time, I poured upon a pound of the same calcined shells, 8 pounds of boiling water.

3. TUESDAY, at eleven before noon, I poured fifty ounces of the lime-water N° 2, on nine ounces fresh calcined oystershells; and, at eight in the evening, I filtered, through brown paper, these three waters, and put sixteen ounces of each of them into a basin by itself; and, having placed the basins in a closet, where they might be pretty free from dust, I let them stand 19 days. After this, I filtered the several waters through brown paper, and having collected the earthy crusts, and dried them well; I found, that N° 1. afforded very near four grains, N° 2. near 12 grains, and N° 3. rather more than 13 grains.

ALTHO' these three lime-waters had, at the time they were filtrated, quite lost their taste; yet, observing that N° 2. and 3. became turbid when mixed with salt of tartar, I added eight grains of this salt to twelve ounces of these two waters; and the white powder which was precipitated, when dried, weighed just one grain and a half.

HAVING

HAVING filtered the lime-waters of N° 2. and 3. into the same bottle, before I suspected that any thing of the lime remained in them, it became impossible to know, which of them afforded most of the calcarious powder precipitated by the alkaline salt, or whether it did not proceed wholly from N° 3.; in which case, sixteen ounces of it must have contained 17 gr. of the earthy part of the lime, and N° 2. only 12 gr.

SINCE N° 2. and 3. were not quite free of the lime, altho' they had stood exposed to the open air 19 days, and had lost above $\frac{1}{4}$ by evaporation; it follows, that the surest way of knowing the quantity of calcarious earth, contained in lime-water, is to evaporate it, as Dr. *Langrish* did*: and, if it be objected to this, that all water affords some earth, when evaporated; the quantity of this may be determined by experiment: tho', in many waters, it may well be neglected, on account of its smallness.

IT has been argued that quick-lime must, after many repeated affusions of water, yield as strong lime-water as at first; because, as long as there remains any virtue in the lime, the

* Physical experiments on brutes. p. 11.

the water will extract it, and continue to do so, till it has taken up as much of the lime as it can bear. But to this we cannot agree: for, tho' there is undoubtedly a certain degree of strength which lime-water can never exceed; yet, in order to communicate to water, this degree of strength, slaked lime may not only be insufficient, but repeated additions of quick-lime may be necessary; unless perhaps a very small proportion of water is poured upon it. Quick-lime, fresh from the fire, yields its virtues more easily, than when weakened by long exposition to the air, or by many affusions of water: the water must extract the virtues of the latter, while the former, by a sort of explosive force of its own, quickly impregnates the water. Nor is it to be wondered at, that quick-lime, fresh from the fire, should, at first, impregnate water more strongly with its virtues, than it does afterwards. This is as easily conceived, as that boiling water should extract more of the virtues of tea or coffee, than cold water. The only difference is, that the *menstruum* in the latter case acts more powerfully, while in the former the substance to be extracted

affords

affords its finer parts more readily, and in greater abundance.

UPON comparing the experiments N^o I. with those of N^o III. and IV. it appears, that the difference between the specific gravities of different lime-waters and common water, is much more than the weight of the calcareous matter contained in these lime-waters: there must, therefore, be something else besides this earthy matter, which quick-lime communicates to water, by which its weight is increased*. Perhaps quick-lime may also, in some other way unknown to us, alter the specific

* As lime-water, after its earthy part has been precipitated by an alkaline salt, continues to taste strongly of the lime; it follows, that, besides this earth, it contains some more active and subtile part, to which its taste and virtues are chiefly owing: for we know that the calcarious matter of lime-water, is perfectly insipid and void of any other virtue, than what all absorbent earths possess. This active and more subtile part of lime-water, seems to be separated from its earth by the alkaline salt, which strongly attracts and embraces it. And hence lime-water mixed with salt of tartar, does not lose its taste of the lime by being exposed to the open air. Does not a solution of a fixed alkaline salt in water, poured on quick-lime, separate this subtile active matter of quick-lime from its earthy part, by strongly attracting it? And do not soap-leys consist of water and a fixed alkaline salt united with this active part of quick-lime, without any, or almost any, of its earthy part?

specific gravity of water. But, whatever may be in this, it is evidently unreasonable to deny that lime-water is as much specifically heavier than common water, as the hydrostatic balance, or other accurate experiments shew; because we cannot account for this excess of gravity from any thing we know of the contents of lime-water. This is no less unphilosophical, than if, one was to doubt of *universal* gravity, because philosophers have hitherto attempted, in vain, to account for it. If we mistake not the matter much, the contrary has always been the opinion of mankind, *viz.* that every well attested fact is to be believed, altho' we are ignorant of its cause, or cannot shew the particular way in which it happens.

ENOUGH, it may, perhaps, be thought more than enough, has been said, to shew that the strength of lime-water is very different, according to the different quantities of water poured on quick-lime. However, I must be allowed to say, that this point, which has been disputed by my good friend, is of that consequence, as to deserve to be fully cleared up; since, to such as drink lime-water, with a view to the cure of the stone,

stone, it is of no small importance to know, how it may be prepared, so as to have the surest and speediest effects. And, as lime-water, injected into the bladder, will undoubtedly dissolve a stone lodged there; it is evident, that, after the bladder has been accustomed to the weaker lime-waters, or to these even softened with a little sweet milk, the dissolution of the stone may be much hastened, by injecting such as are more strongly impregnated with the virtues of the lime.

WITH regard to the lithontriptic powers of oyster-shell and stone-lime water, I shall only say, that, as in a variety of experiments made during the course of ten years, I had always observed the superior efficacy of the oyster-lime-water; I thought it to no purpose to make a new trial: any one, who doubts on which side the truth is, may easily satisfy himself. But, in making the experiment, the *calculi* should either be immersed in a large quantity of lime-water, or else it should be renewed upon them every three or four days.

ART. XIV.

Of the anthelmintic Virtues of the Root of the Indian Pink, being Part of a Letter from Dr. JOHN LINING Physician at Charlestown in South Carolina, to Dr. ROBERT WHYT, Professor of Medicine in the University of Edinburgh.

—THERE is a plant, commonly called, by the *Carolinians*, *Indian Pink*, which grows in this province, the root of which is used as an anthelmintic, and was first communicated to the *English* by the *Indians*.

THE root is either given in powder, or an infusion is made of it in boiling water: but the powder is most effectual. When I give the powder, I add some rhubarb (a sufficient quantity to keep the belly open) and a little of some of the essential oils, as the *ol. rut. sabin.* or *absinth.* To a child of three years of age, 12 grains of the root in substance is a moderate dose: and I repeat it morning and evening for some days; ordering, at the same time,

time, a proper regimen. When the root is given in infusion, 20 grains of it make a sufficient dose for a child of the same age.

It has an advantage above all other anthelmintics that I am acquainted with, as it is less nauseous, and may safely be given in fevers without heating too much: likewise children, when they awake in the morning, may be deceived with it, by giving the infusion with a little milk, and a sufficient quantity of sugar, in place of tea. Tho' it does not always succeed in expelling worms; yet I have observed, that children's complaints were remarkably relieved after having taken it for some days.

I shall only trouble you with one short history, where I gave it the other day with success. I was sent for to a Negro-child about four years of age, on the seventh day of a continual fever: and, as I suspected that the fever was kept up by the irritation of worms, I ordered the following, that morning; R. *aq. theriac. drach. ii. aq. font. unc. iijss. pulv. rad. anthelmiae* (for so I called the Indian pink) *tart. regenerat. ana scrup. i. pulv. croci anglic. gr. v. sacch. q.s. M. sumat coch. unum secunda quaque hora.* Next morning, I ordered

a repetition of the same ; and, in the afternoon, when the child had a stool, 30 large worms, the *teretes*, were at once voided ; and that afternoon, the fever went intirely off, and did not return. Next morning I repeated the same mixture ; and, that day, the child voided 9 more of the same kind of worms.

WHEN too great a dose of the powder or infusion is given, it has some strange effects, which appear very soon after the dose is taken. Children become vertiginous, complain of a pain in and over their eyes, and the *musculi adductores* and *abductores oculorum*, seem to be greatly affected, from their irregular actions ; while the other muscles of the eye, so far as I have had an opportunity of observing, remain unaffected. These symptoms, however, soon go off ; and their removal may be hastened by giving some weak spirituous liquor, to which volatiles or saffron, or any of the warm essential oils, are added. But, to prevent any of these bad effects of this medicine, some gutts of the oils before mentioned are added,

I have been the more particular in my account of this medicine, as I imagine it may, when properly introduced into practice, be of great use in saving the lives of many. It has been, for many years, used in this part of the world, not only by all the practitioners, but likewise universally by the planters.

ART.

ART. XV.

The History of a Cure performed by large Doses of an alterative mercurial Medicine, communicated to Dr. PLUMMER; by Mr. GEORGE DENNISTOUN Surgeon in Falkirk.

ABOUT the month of *December* 1737, a gentleman applied to me for cure: he complained, that, for some time past, he had felt pricking pains in several parts of his body, especially about his shoulders and shin-bones, which were now become intolerable. He had dry scurfy scabs over the whole inside of the thighs, the glands of both groins were a little tumified; and hard crusty scabs were here and there scattered upon the *scrotum*, hips, and about the *podex*. From these, and other circumstances, I had no reason to doubt that my patient's distemper was the venereal lues; upon which, I raised a salivation with *merc. dulc.* in the course whereof, all the symptoms gradually disappeared. I then put him upon a course of the *pil. Æthiopic.* and

and decoction of the woods; by which means, he seeming to be very well, I flattered myself that the disease was intirely eradicated. He had no complaints for above twelve months; but then he began to feel the old pricking pains, especially in his legs; and, in a short time, he discovered a swelling on the most prominent part of his shin-bone, about the bigness of a walnut, tho' flat; with such scabs, &c. as were formerly mentioned: and all this (as he told me) without reason to suspect a new infection.

IN short, he applied to me a second time, and, having advised with a physician, very eminent in his profession, he desired me to put him upon a new course of *merc. dulc.* but to give such small doses, at such intermediate distances, as to raise the salivation very slowly; and, when once raised to the quantity of lib. iii. or lib. iv. in a day, to keep it up at that rate, by now and then dropping in a bolus, for two months at least; and, at the same time, to annoint the node on his shin-bone with *ung. mercurial.* &c. I followed his directions punctually; and, indeed, all the symptoms went gradually off, except the forementioned node, which was little or nothing

thing altered, only he felt no pain in it : I was then ordered to give him the *pil. Æthiopic*; and *decoct. lignor.* after the use of which, for above a month, he began to feel some stinging pains in the node, and even through the whole leg, which daily increased. I had read the history of your alterative pills, some time before : and, judging they might be more forcible than the *pil. Æthiopic.* in removing these symptoms, and the remains of the distemper ; I hinted the same to the Doctor, and he chearfully agreed to use the one in place of the other. I shall here observe that the first salivation was raised with *scrup. iv. of merc. dulc.* and that he only swallowed *scrup. viii.* through the course of the other; by which it would appear that that medicine, had by no means become familiar to him. I prepared your pills precisely as directed in Medical Essays vol. i. art. vi.; only instead of *calomel*, I took the same quantity of *merc. dulc.* and indeed of the same kind with which I had raised the last salivation : and, having divided the mass, as is there also directed, I ordered him to take three pills morning and evening with a draught *decoct. lignor.* after each dose, and to drink plentifully of

the

the same thro' the day. This course, with a due regimen, he observed regularly for a month : but as he was quite wearied with the tedious course he had formerly undergone ; and, as he afterwards told me, was resolved his disease should terminate soon, either in death or recovery ; he frequently begged I would increase the dose of pills : upon which I allowed him to take four, morning and evening, but told him I durst venture no further ; and withal what quantity of mercury they contained : he at the same time asked me, what would be the effect of over-dosing them ? I told him, I believed they would either cause him vomit, or raise a salivation : this he pocketed up, as being signs sufficient to know when to stop : in short, as he had access to get the pills renewed at my shop when he pleased, without my knowing any thing of the matter ; he had taken 24 pills each day for the continued space of six weeks, without being attended with the smallest inconvenience : and in that time the erratic pains and node had intirely vanished : but he afterwards made use of the pills, tho' in smaller doses. Having thus conquered his disease by his rashness ; he triumphed as ha-

ving also conquered the too timorous caution of his physicians. Upon a calculation, I find he had taken *unc.* xiv. of the alterative pills, which may contain about *unc.* iii. of sweet mercury, from the first day of *December* 1739, to the first of *May* immediately thereafter; but that he had taken *unc.* ix. (consequently above *scrup* xv. of sweet mercury) within the space of six weeks without intermission. And ever since he has enjoyed perfect health. From this accidental experiment, I conceived such a good opinion of those pills, that I have frequently used them since, in many different cases; and with much success: I have found most cutaneous diseases yield very soon to this remedy; amongst others, what is vulgarly called *Sibbens*, which rages in the moorlands near this place: whether this distemper is really a pox, I shall not pretend to say; only it is attended with the same symptoms: and I now begin to think, that, if these pills are given in such large doses as before mentioned, in which, I accidentally found there was no danger; I don't doubt, but most of the diseases which usually yield to mercury, may be subdued by this medicine. I shall only add

add that I now omit the *resina guajac.* and increase the quantity of the gum ; and I boil the *lign. guajac.* much longer than is directed in the *Edinburgh Dispensatory*, which, I am sensible, makes the decoction much stronger.

ART.

ART. XVI.

A Description of the seminal Vessels, by ALEXANDER MONRO, Student of Medicine in the University of Edinburgh.

THO' all anatomists agree, that the seed separated in the testicle, passes thro' the *epididymis* to rise in the *vas deferens*; yet few have a right notion of even the grosser tubes in which it is conveyed. I have luckily made quicksilver pass farther in these seminal vessels, and thereby brought more of them into view, than I see described in any anatomical book; as you may judge by the preparations and pictures which I now send you, at my father's desire.

De Graaf had formerly endeavoured, by dissection, to unravel the *epididymis*; and paints it *, as rising from the testicle in six or seven branches, which join at its *caput* or first remarkable turn into one pipe, the numerous contortions of which form the body of

* *De Vir. Organ. Tab. 1. Fig. 3. et Tab. 4. Fig. 3.*

of the *epididymis*, terminating at its other end in the *vas deferens*.

THIS seems to have been overlooked, or not well understood, by most of the modern anatomists, who have differed widely, or talked with uncertainty, about these pipes; till of late, that the ingenious Dr. *Haller*, by injecting quicksilver from the *vas deferens*, in the manner proposed by my father in the *Medical Essays* *, and causing it to pass as far as the testicle, has been able to explain to us, with greater accuracy, the structure of this intricate organ †. He agrees with *De Graaf*, that the *epididymis*, from the *vas deferens* to its *head*, seems to be composed of a single pipe, which he thinks might possibly be unloosened, as *De Graaf* has represented; but does not affirm his having executed it. At its *head* indeed, he could divide it into ten or more vascular cones, from which vessels go out, that, after forming a network with communications, give off streight pipes which seem to plunge into the body of the testicle. —His injection here generally failed, tho' he sometimes observed, that it entered a few convoluted

* Vol. v. Art. xx. § 29.

† Phil. Trans. No 494. § xii.

voluted tubes.—He mentions his having thrice seen a vessel going off from about the middle of the *epididymis* towards the spermatic cord, which he imagines is a *lymphatic*.

THAT this accurate anatomist's improvements, and the additions I am to make to them, may be better understood; I have first copied his elegant figure, and then subjoined some others I caused to be drawn of those parts.

TAB. 3. fig. 1. from Dr. Haller.

A. The *vas deferens*.

B. The end or last turn of the *epididymis*.

C. Its body, as it appears when the external membrane is dissected off.

D. Its *head* unloosened.

eee. &c. The vascular cones into which it divides.

ff. The *vasa efferentia*, as he calls them.

gg. The network with communications.

bb. The streight vessels which seem to plunge into the testicle.

In fig. 2. The *tunica vaginalis* of the testicle is cut off, and the testicle, injected *epididymis* and spermatic cord, are represented covered with their membranes.

A.

A. The serpentine beginning of the *vas deferens*.

B. The *globus minor*, or last turn of the *epididymis*, from which the small convoluted vessel, *c*, which Dr. Haller supposes to be a lymphatic, rises.—If it is so, it has not the valvular structure common to those vessels; for I can cause the quick-silver to flow either way in it.

D. The *corpus pyramidale*.

E. The body of the *epididymis*, where the convolutions seen thro' the membranes, are larger than they appear in *fig. i*.

F. The *head*, *globus major*, or first remarkable turn of the *epididymis*.

G. Its rise from the testicle.

In *fig. 3.* and *4.* The *tunica albuginea* is taken off from the sides of the testicle, and turned over upon the *epididymis*.

A. The *tunica albuginea*.

B. The testicle.

bb. Dr. Haller's streight pipes from which

ccc. The small convoluted tubes upon the sides of the testicle are continued.

dd. In *fig. 4.* Represents a number of them upon the convex part of the testicle, at the greatest distance from the *epididymis*.

FIG.

FIG. 5. Represents the testicle drawn asunder in its middle from its convex surface towards the *epididymis*.

FIG. 6. A small piece is in the same manner separated from the side of it.

IN both we observe Dr. *Haller's* streight vessels entering at *bb*; and then dividing into *ccc* innumerable convoluted pipes, which compose almost the whole substance of the testicle.

dd. A number of them where the injection has gone very far.

N. THESE preparations had been kept several months in spirits, and so often handled before the drawings were made, that many of the injected vessels are destroyed, and the painter could not distinguish the membranous partitions of the testicle.

As many of the most accurate anatomists, and even *Winslow* *, have imagined, that the body of the *epididymis* is composed of a number of pipes.—As Dr. *Haller* does not positively affirm his having unloosened it.—As the *vas deferens* is much larger than any
pipe

* *Exp. anat. traité de bas ventre.* § 488.

Fig. 1.

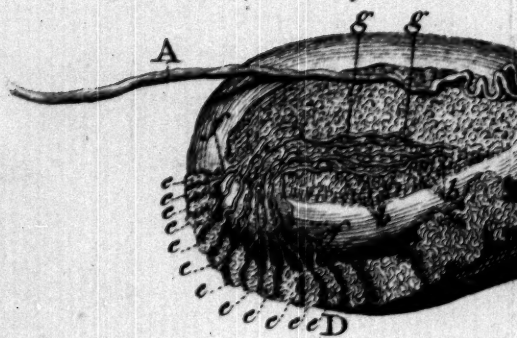


Fig. 3.

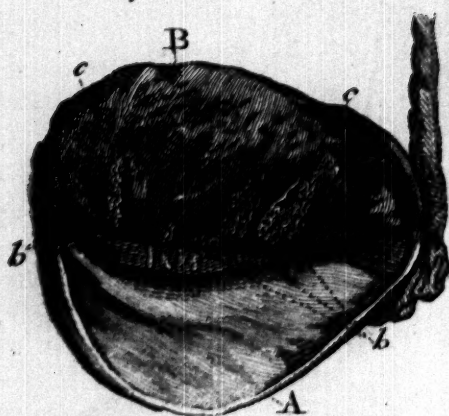


Fig. 4.

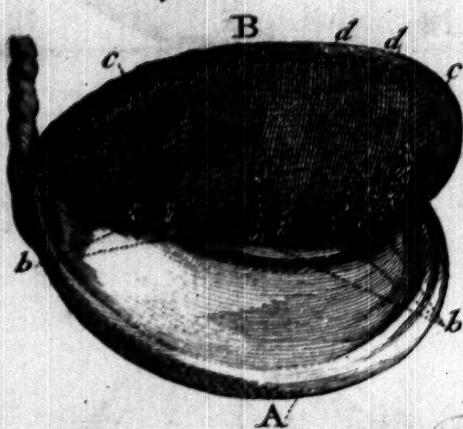


Fig. 6.



B. III.

1.

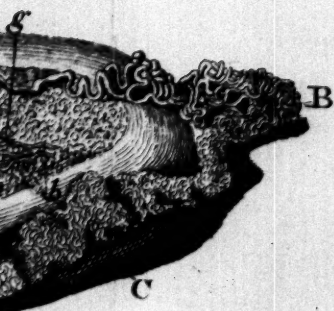


Fig. 2.

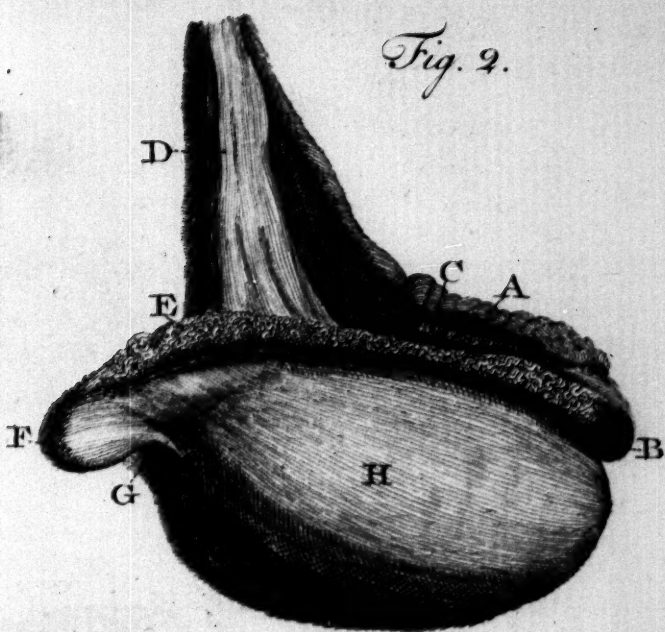


Fig. 5.



6.



pipe in the *epidymis*, and as it seems difficult to conceive how the seed can be propelled thro' a tube so small, so much convoluted, and of such a length as the single one, which, according to *De Graaf*, forms the *epidymis*: on these accounts, I think, there is reason to doubt this structure which he asserts, till we have more satisfying proof of it than has hitherto been adduced.—Such the following appears to be. If, in attempting to make preparations like to those above described, an extravasation happens in any part of the *epidymis*, or if any of its convolutions are tied, not a drop of the injection can be pushed farther towards the testicle than this extravasation or ligature: and, if the *epidymis* is cut across, while quicksilver is poured into the tube fixed in the *vas deferens*, it squirts out only at one orifice in this cut part; which is the plainest and most convincing demonstration of the body of the *epidymis*, being intirely composed of a single tube convoluted in a most wonderful manner.

Tho' my injection penetrated far into the feminal pipes; yet still we are unacquainted with their origin. *De Graaf** attempted in

E e e

vain

* *De vir. org.* p. 42.

vain to discover this by maceration in water; nor have anatomists since his time been more successful.

I have endeavoured several times to force quick-silver from the spermatic arteries into the feminal vessels; but, tho' I made it return by the spermatic veins, I could never cause it to pass into these convoluted tubes.

THIS I attempted the oftener, because of the seeming probability of our being able to determine, better in this than in other glands, whether secretion is performed in continued cylindrical canals, according to *Ruysch*, or, by the intervention of follicles, with *Malpighius*; since we can separate the parts of the testicle, without cutting, and even almost without laceration.

A R T.

ART. XVII.

*The Dissection of a Woman with child; and
Remarks on gravid uteri, by Doctor DONALD
MONRO Physician at London.*

GENTLEMEN,

THE few histories of dissections and figures there are of *gravid uteri*, make me imagine, that an account of what I observed in one that fell under my examination, illustrated with a few figures which I took from the subject itself, would not be unacceptable to you.

WHILE I was at *Edinburgh* last year, the body of a woman, said to be six months gone with child, was brought to the anatomical theatre; which my father desired me to examine accurately, and particularly to endeavour what I could to shew the *anastomosis* between the vessels of the womb and those of the secundines, if there were any such, as *Nortwyk* * affirms there are, and *Haller*, in his *Primæ Lineæ* †, seems to alledge; tho'

* Hist. uteri human. p. 1. § 7. and p. 2. § 83.

† § 830.

tho' in private conversation with this last named gentleman, when I was his scholar at *Gottingen*, he told me, that, what he had wrote concerning that subject, was upon the authority of others, and not from his own observation; for that he had had three women with child for subjects, whose bodies he had injected and dissected; but had found nothing like an *anastomosis* between the uterine and secondary vessels in any of them: so that he now believed there was no such *anastomosis* as was alledged.

I opened the body of this woman; and, at first, imagined we had been mistaken about her being with child; for the intestines came as low down as the *ossa pubis*, and covered intirely the *uterus*: but, in turning up the intestines, we were soon undeceived, and saw the distended womb.—I then laid bare the descending *aorta*; and, having fixed a large pipe of a syringe into it, and warmed the *uterus* well, by pouring warm water upon it, and by keeping afterwards applied to it sponges filled with the same; I injected hot oil of turpentine, coloured with very fine powder of vermillion, into the *aorta*; and, after that, a coarser injection

of

of melted tallow, diluted and coloured with part of the former injecting materials.—My father then demonstrated to his pupils the parts in their natural situation; and I made the sketch of the *uterus*, as represented in *tab. iv.* where the containing parts AAAAA, being laid aside, you see the womb BBBB, with its *fundus*, raised higher in the right side than in the left;—CCC, the larger branches of the arteries on the foreside of the womb of a very serpentine form, and anastomosing with each other;—DD, the round ligaments; EE, the Fallopian tubes fixed to the *uterus*, a good way below the *fundus*, instead of being fixed to its angles, as they are in an unimpregnated womb;—F, the urinary bladder, which, in this subject, rose a good way above the *pubes*, turned down over it with a piece of the integuments;—G, the left *ovarium*.

NEXT day, the substance of the womb was cut thro' cautiously, a little farther forwards than the round ligaments; and, when the incision had penetrated into the *uterus*, a substance appeared which separated quite easily from the more external parts, by being gently pressed with the finger; and, by pushing

ing the finger alternately upwards and downwards, so much of this substance was separated from the womb, as to allow a probe-pointed pair of scissars to be introduced for enlarging the incision, parallel with the side of the *uterus*. I then saw that this substance was a villous membrane, which separated almost of itself from the cut sides of the womb, when we attempted to raise them. In doing of this we could not observe any tearing of vessels or fibres, except in some places where small convoluted arteries were broke off from the forepart of the *uterus* to adhere to this villous membrane. In this manner, this outer *lamella* of the *chorion* was every where separated from the forepart of the *uterus*, which was cut thro' with a pair of scissars all around its *fundus* and edges, as low down as the *ossa pubis*, and was turned down over these bones.

THE *uterus* was, as near as I could judge, about the thickness of an unimpregnated one: some of the gentlemen who were present at the dissection, thought it was thicker, and others, that it was thinner.

THE substance of the womb was composed of its firm strong outer membrane which

is a continuation of the *peritoneum*: this adhered by a cellular membrane, thro' which a number of arteries ran, to a red coloured substance, which was about one half the thickness of the womb, and had a muscular appearance; but I could not observe in it any real muscular fibres, laid in regular *strata*.—Within this there was a whiter coloured substance, which, on stretching, appeared plainly to be a cellular membrane, with its inside smooth and polished; but with a cellular appearance whenever it was stretched, by endeavouring to raise it from the substance of the *uterus*: for the internal very thin membrane, which covers the inside of the womb, and is continued from the external *cuticula*, is so fine, that it is scarce to be distinguished. Numerous vessels, both arteries and veins, ran every where thro' this cellular substance.

In the middle of the cut sides of the *uterus*, the empty veins were of a great size, in comparison to what the arteries, distended with injection, were.

On the internal surface of the part of the womb which was turned down over the *pubes*, convoluted arteries were seen; and some orifices

fices of veins appeared. The outer *lamina* of the *chorion*, was scabrous, succulent, somewhat villous, of a pale fleshy colour, and not at all transparent. The convoluted arteries which I formerly said had adhered to it, when the womb was separated from it, were taken off with a pair of dissecting forceps, without any discernable tearing of fibres. Cross incisions were then made into this outer covering of the *ovum*, and it was laid to a side, when we saw the second *lamella* or clear transparent proper *chorion*, which separated almost of itself, from the outer membrane; they being only connected together by a very fine and tender cellular substance. This internal *lamella* or proper *chorion* being cut thro', the fine *amnios* appeared, thro' which was seen the *foetus* swimming in its waters.

IN TAB. vi. *fig. 1.* are represented AAA, the edges of the cut *uterus*, with large oblong orifices of the cut empty collapsed veins, and of the smaller round injected arteries;—B, the forepart of the *uterus* turned down between the thighs;—CC, part of the outer *lamina* of the *chorion*;—DD, part of the inner *lamina* of the *chorion*;—E, the *amnios* unopened,

opened, thro' which is seen the *foetus* in its waters.

THE membranes being all cut thro', and the waters let out, we more plainly observed, as in TAB. v. *fig. 1.* the *fætus* A, with its head downwards; B, the navel-string twisted round its neck;—the *placenta* CC adhering to the back part of the womb, with the membranes DDD, lying folded upon its edges; and the cut sides of the *uterus* EEE, where the orifices of the veins and arteries appear, as in the figure formerly referred to;—F, the *vena cava*;—G, *aorta descendens*;—H, the inferior mesenteric artery, to shew how high the *fundus* of the womb was raised.

THAT a comparison of the situation of this *fætus*, with others, might be made; I sketched in miniature the outlines of *Bidloo's*, and *Albinus's* figures. *Fig. 2.* of TAB. vi. is *Bidloo's*, where the *fætus* lies in a bended posture, with its head uppermost, and its *placenta* adhering to the forepart of the womb. And, *fig. 3.* of the same table, is copied from *Albinus*, where the *fætus* is with its head more perpendicularly downwards than in my subject; and no more, than the edges
F f f of

of the *placenta* adhering to the back part of the *uterus*, can be seen.

WE fought diligently for an *allantois*; but could see nothing that bore any resemblance to it.

THE following day, tallow diluted with oil of turpentine, was thrown into the umbilical arteries towards the *placenta*; and some of the same materials, coloured with *rad. anchusæ*, was injected into the umbilical vein.

—Tallow, diluted with oil of turpentine, and coloured with verdigrease, was likewise thrown into the uterine veins, after its cut edges were all tyed.

THE *fœtus* being taken out, we saw how far down the *placenta* adhered to the womb, as in TAB. v. *fig. 2.* where AA, is the lowest part of the *placenta*, with some of the membranes turned upon it;—BB, a cluster of *sinuses*, which appeared where these membranes had been taken from;—C, the entry to the *cervix uteri*;—D, a share of the forepart of the *uterus* turned down.

UPON taking the *uterus* out of the body, and opening the back part of the *vagina*, there was, as in TAB. iv. *fig. 3.* A, a considerable part of the *cervix uteri* very little distended;

distended; and the *os tincae* B, appeared like a small *rima*, beset all around with a glandular ring, which was filled with a thick *mucus*,

AFTER this, the upper half of the *placenta* was separated from the womb, to which it adhered, by a cellular membrane of a stronger appearance than what connected the womb to any other part of the *ovum*.

EXTRAVASATED injection of all the four different kinds, was found between the *placenta* and *uterus*; the greatest part of it, however, was of the vermilion injection, that had been thrown into the descending *aorta*. Tho' the extravasated injection had made its way into the cellular substance both of the *uterus* and *placenta*; yet not one single vessel of the *placenta* was filled with any of the injection that came from the *uterus*; nor was a vessel of the womb filled with that which came from the *secundines*.—In some places where there was none of the extravasated injection adhering to the *placenta*; I observed that it was covered with the outer *chorion*; and some arteries broke off from the womb, adhered to it, but were taken away without laceration.

MOST

Most of the apertures commonly called *sinuses*, were full of the green injection; and some few of them had a very little of the vermilion in them. The internal membrane of the *uterus* was extended over part of each of their orifices, and there formed a sort of valve.—Some of these orifices were so large, as to have allowed one's little finger to have been put into them; while others were so small, as scarce to admit the point of a probe: and there were of all the different sizes between these. — The veins continued from them were of a considerable diameter; but not so large as the *sinuses* themselves.

THE extravasation having made the contiguous surfaces of the *placenta* and womb of my subject less distinct than I could have wished; I drew small figures of these parts, from *Albinus's* elegant large original ones. *Fig. 4.* of *TAB. vi.* is the *placenta*, exhibited with its surface contiguous to the *uterus* in view; where the engraver has not done justice, in representing the *monticuli* and *fulci* of the *placenta*.

Fig. 5. is better copied, where the oviform figure of the *uterus* of his subject, strikes the eye, with the orifices of the cut vessels in its substance,

substance; and the insertion of the round ligaments are seen, at AA. The numerous *sinuses* sufficiently shew where the *placenta* adhere to it. To some few of the valvular membranes of those *sinuses*, I have put *a*; and *b* is placed near to what represents their orifices;—*cc*, point some of the serpentine small arteries;—D, the valvular protuberance at the back part of the *cervix uteri*;—*e*, the *os tincae* with part of the *vagina* opened.

TAB. vii. *fig. 1.* Is the appearance which a part of the *uterus* of my subject had, where the *sinuses* were not distended; and some of the serpentine uterine arteries were distinctly seen.

Fig. 2. A *sinus* A, with its valvular membrane cut away, when the orifices of three veins B are seen.

Fig. 3. The three veins slit open, till where they meet in one trunk.

Fig. 14. Is a copy of a little piece of *Albinus's* figure, where the *sinuses* and serpentine arteries are represented of the natural size.

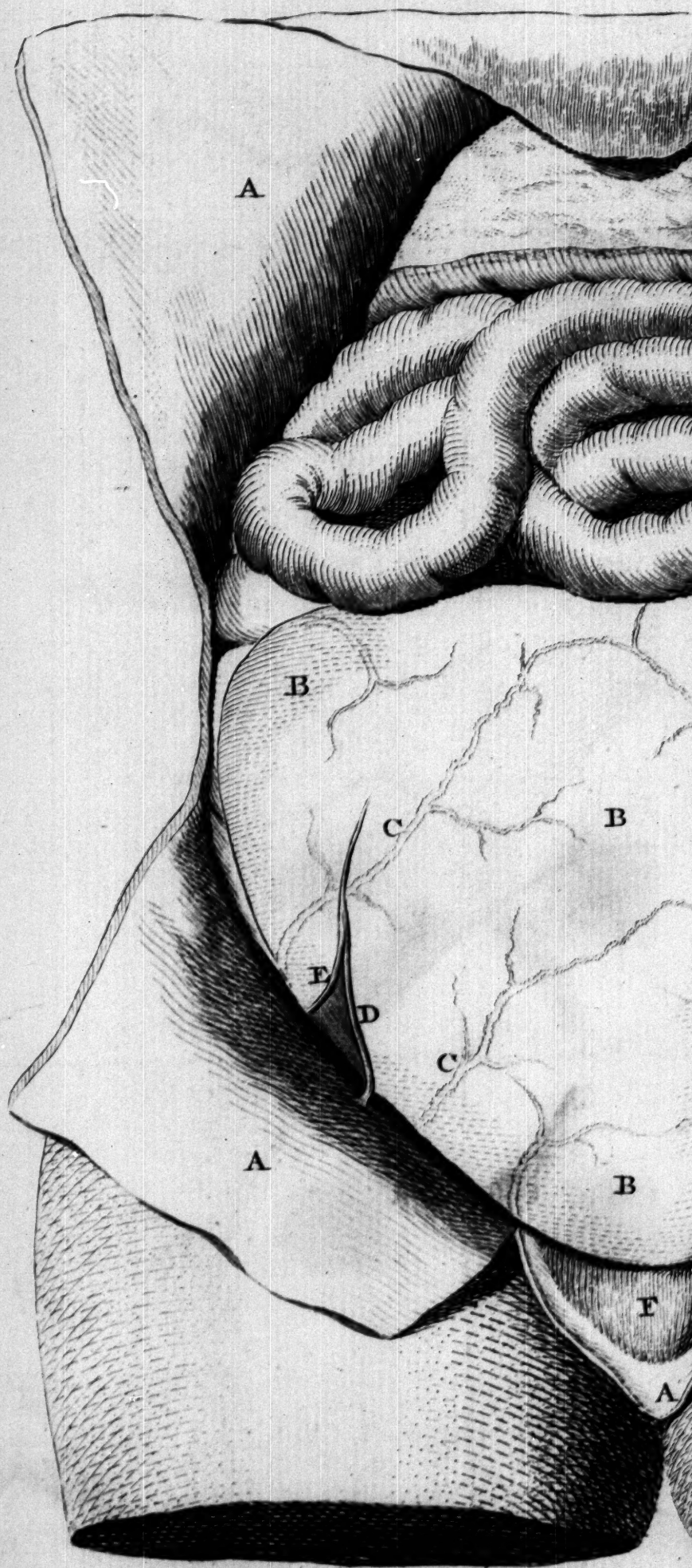
THE spermatic vein appeared of such a monstrous size, and so much larger than the spermatic artery, that I dissected and made a draught

draught of them, as represented in *fig. 5.* where the vein AA, and the artery BB, appear twisted frequently round each other; and sometimes as if the artery entered into the vein;—C, the *fimbriae* of the *tuba Fallopi-ana*;—D, the *ovarium*;—E, part of the *ligamentum latum*.

THAT others may judge how long my subject had been with child, I have added the outlines of the *foetus* of the natural dimensions, in *fig. 6.*

THUS, Gentlemen, I have fairly related all that I observed in the dissection of this pregnant *uterus*; and beg leave to be allowed to add some few remarks upon the whole.

FROM the figure of the womb I dissected, compared with those of *Cowper*, *Nortwyk*, *Albinus*, *Hunter* and others, it appears, that the *fundus* of the impregnated womb, is always greatly extended upwards, in the time of gravitation, by which the round ligaments and Fallopian tubes, which are inserted into the angles of the unimpregnated womb, are then much lower than the *fundus*: for, in my figure, these ligaments and tubes, are almost one third of the length of the womb
lower



B. IV.

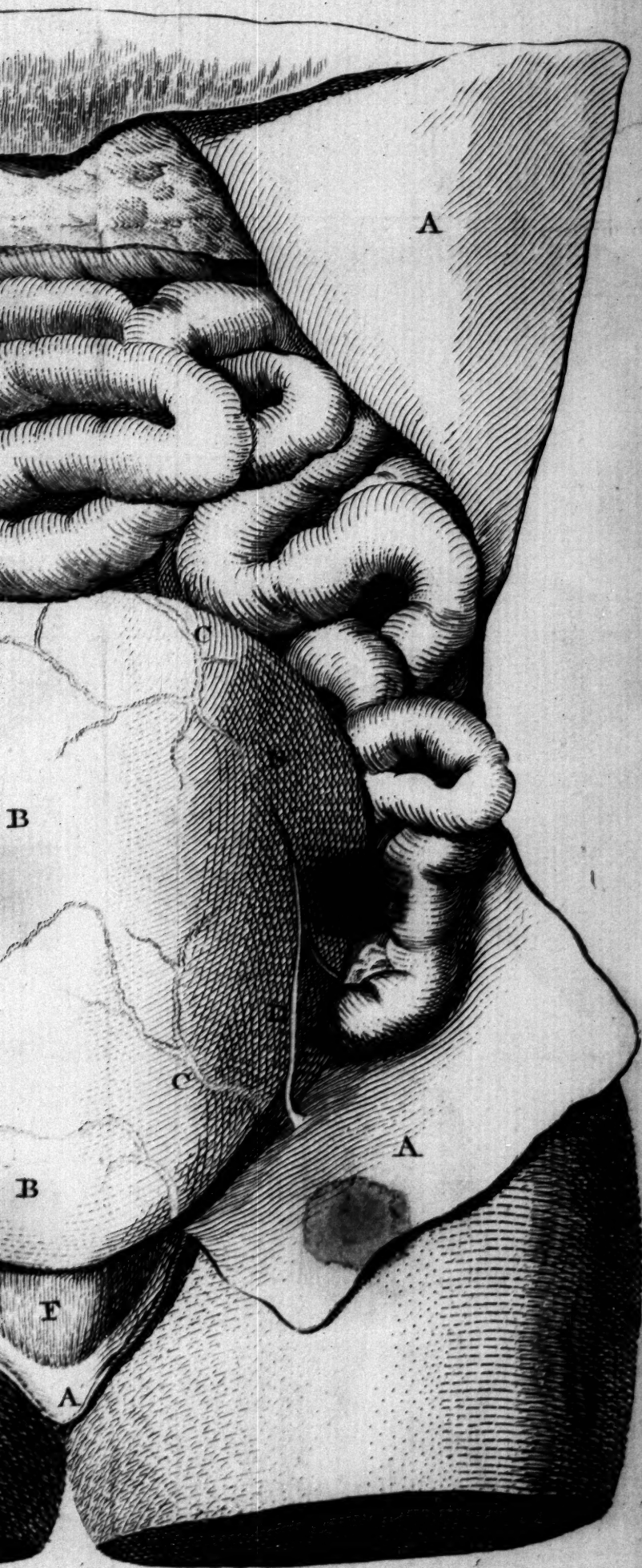


Fig. 1.

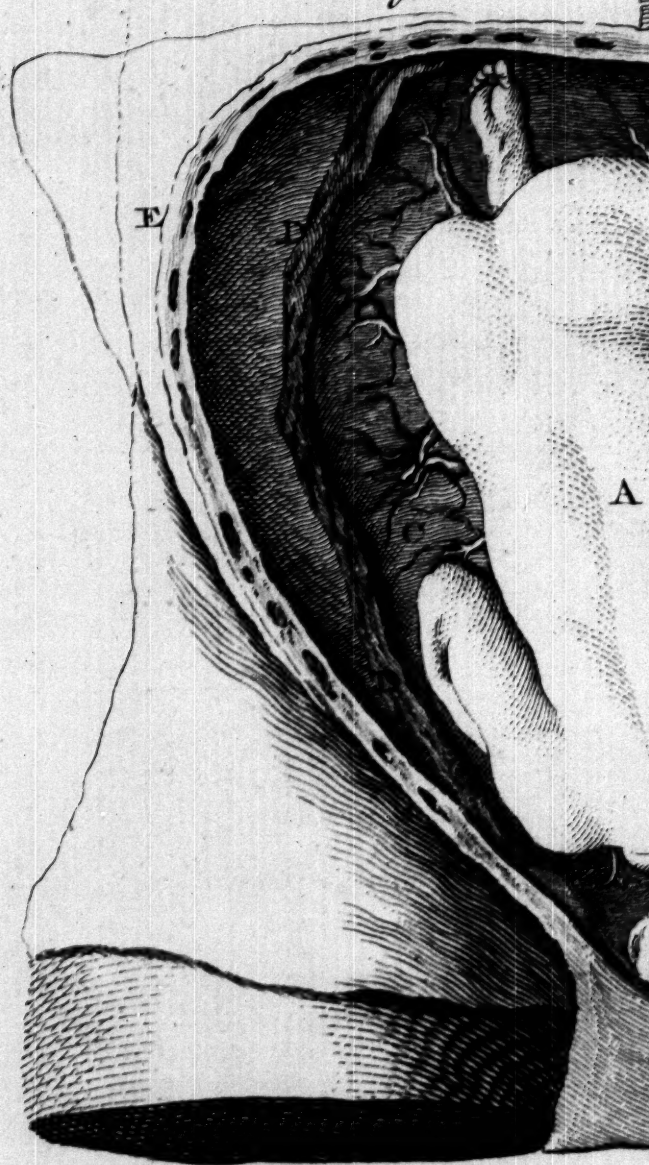


Fig. 3.

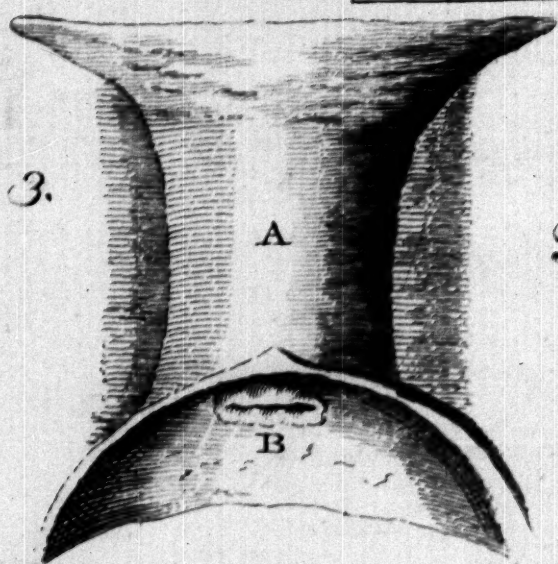


Fig.

TAB. V.

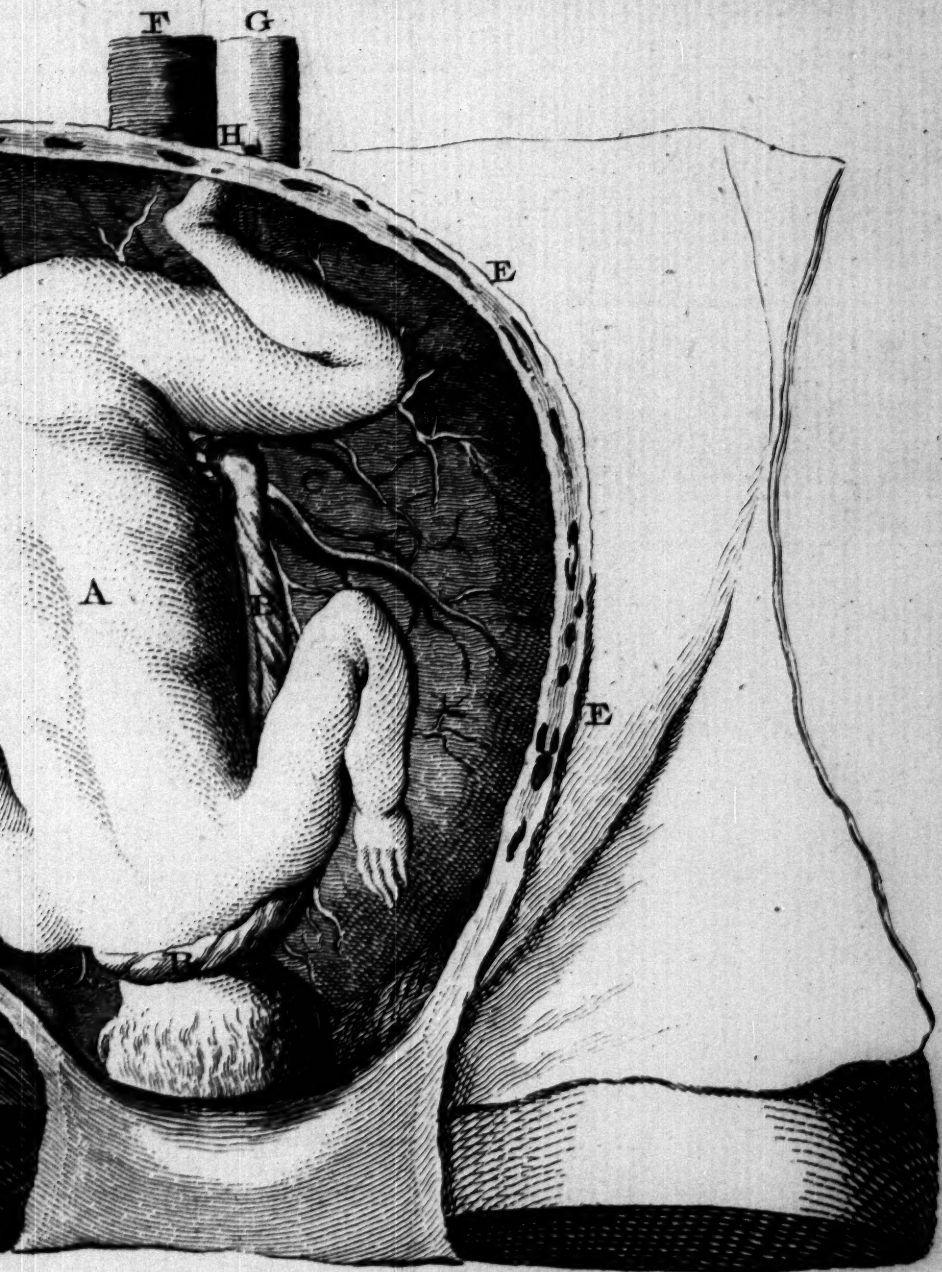


Fig. 2.

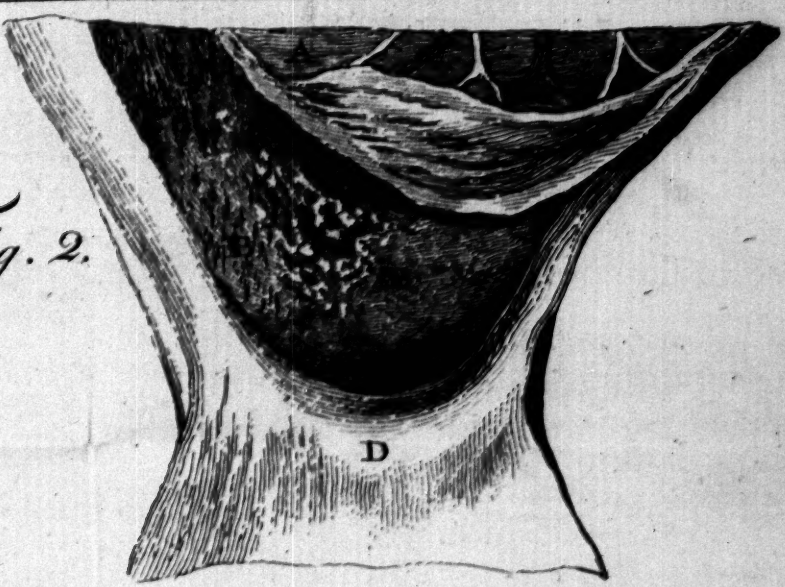


Fig. 2.



Fig. 3.

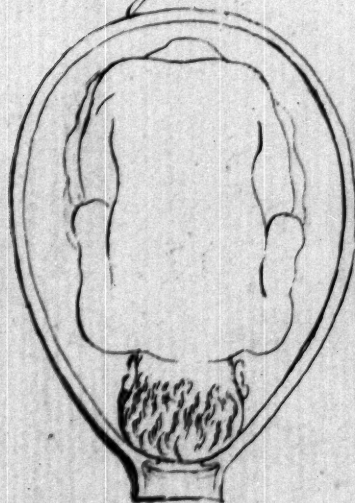


Fig. 4.

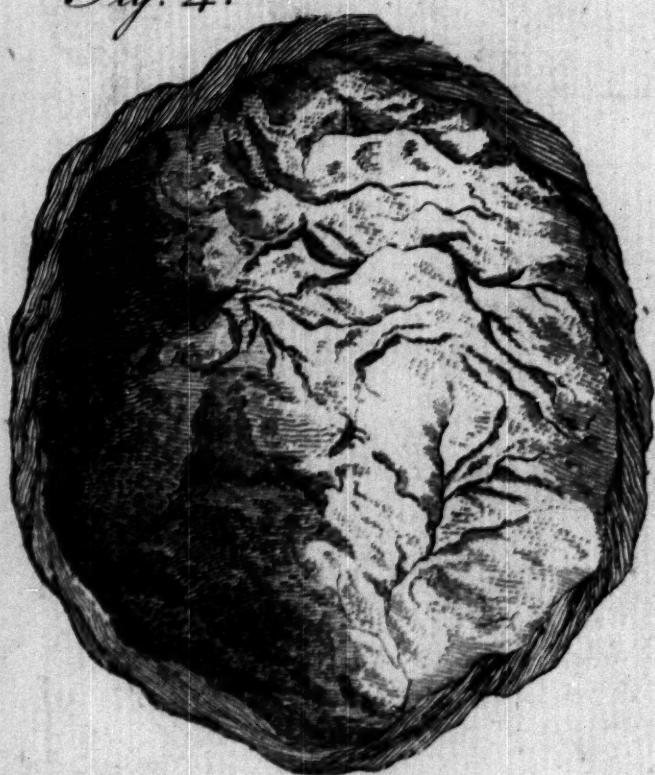


Fig. 1.

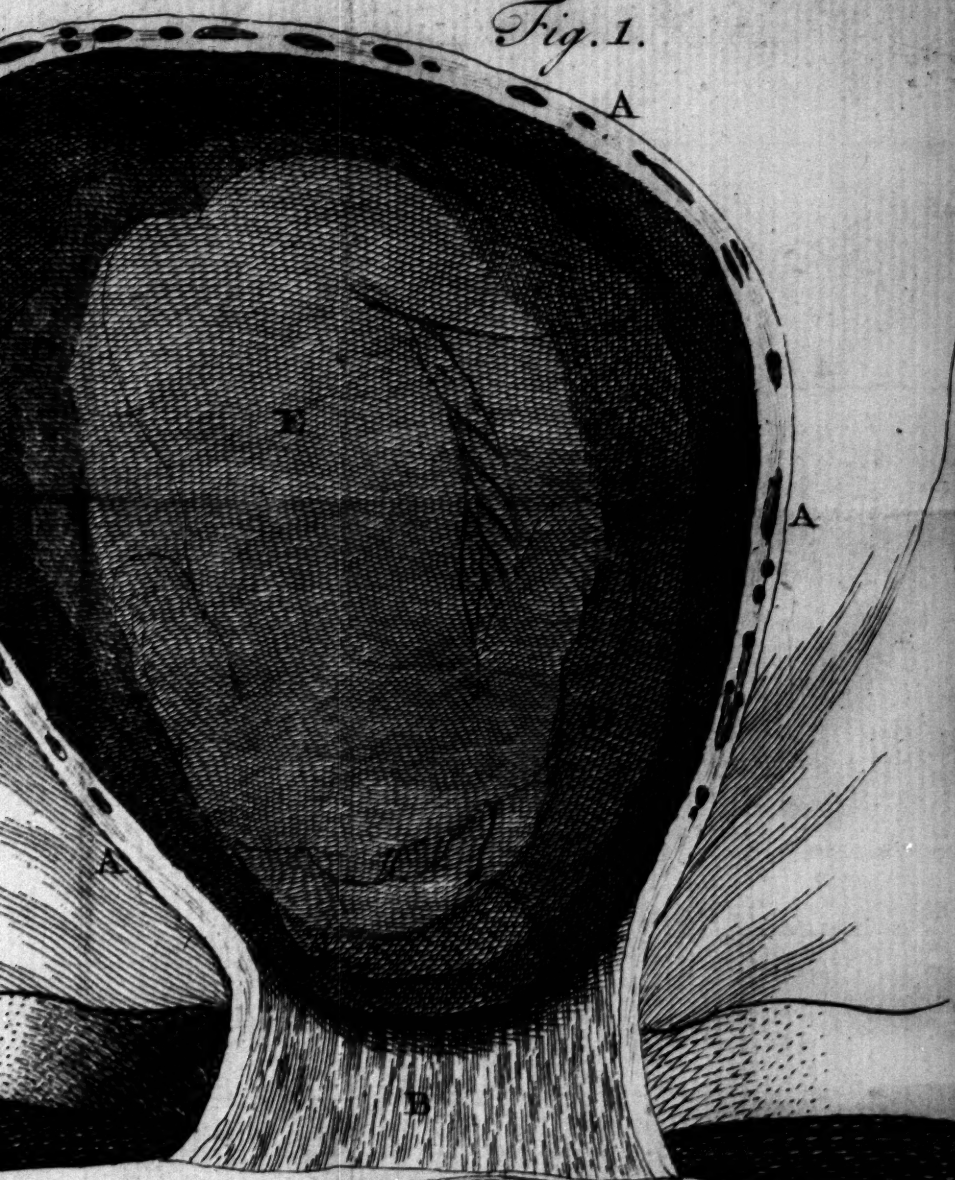


Fig. 5.



Fig. 1.

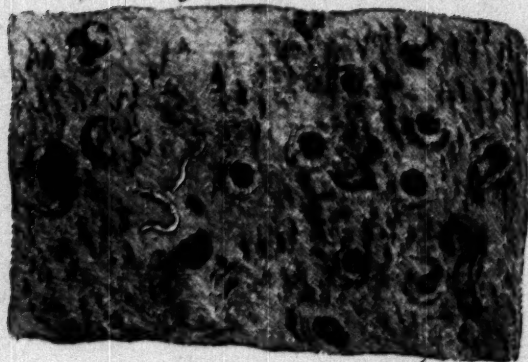


Fig. 3.

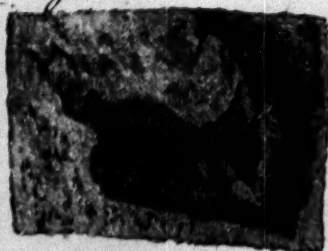


Fig. 2.



Fig. 4.

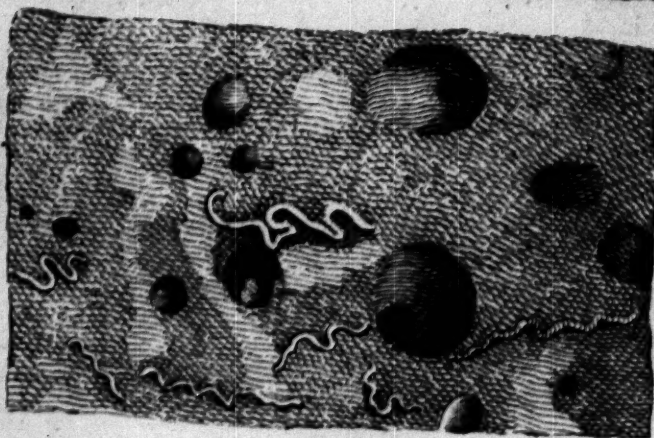


Fig. 7.

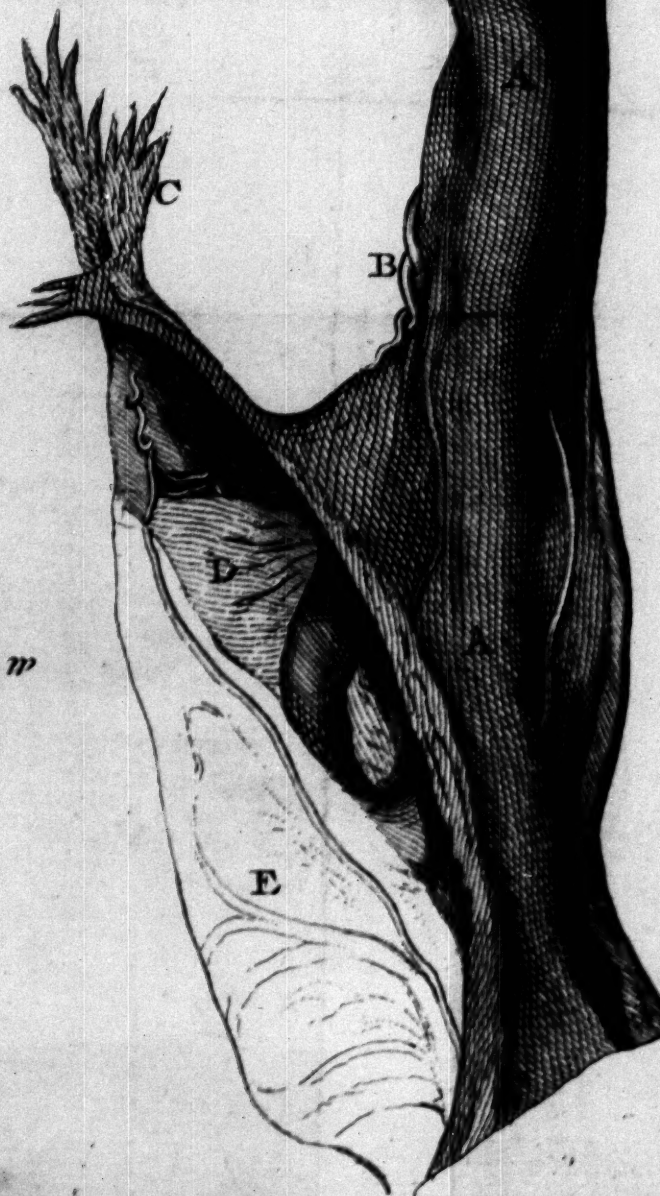
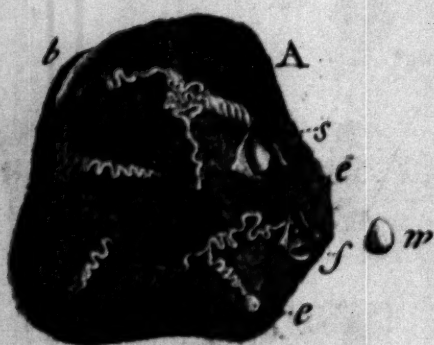
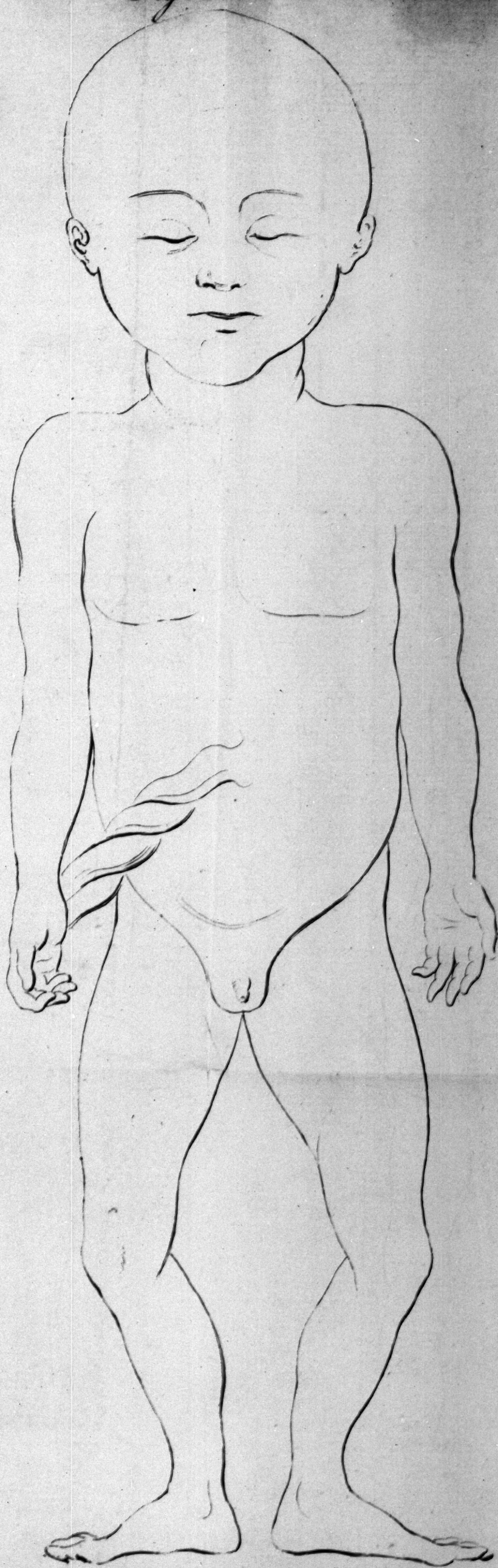


Fig.



lower than its top: and *Vesalius* *, *Nortwyk* † and others, have observed, that the further advanced the woman is in her pregnancy, the lower they descend.

THE *uterus*, by impregnation, may be forced into different shapes. My figure and that of *Bidloo*, which resemble each other greatly, shew, that one side of the womb may be raised much higher than the other. *Vater* and *Albinus*, have painted the *uterus* as oval; and *Nortwyk* has represented it flat at top: but, as all these three Gentlemen's figures were done from wombs which were previously taken out of the body, we can form no judgment of the shape of the womb from their draughts; because when the *uterus* is taken out of the body, it assumes a different shape, according to the manner in which it is laid down. In Mr. *Hunter*'s elegant figure, which was taken from the *uterus in situ*, the womb is more oblong than in mine. And in two other pregnant women I saw dissected, the one seven, and the other four months gone with child; the *uterus* was in both more globular

* lib. 5. cap. 17.

† Hist. Uteri, pars 2. § 77. p. 70.

globular than in the present subject. Possibly the figure of the womb may gradually become more oval, as the woman approaches to her time. The part likewise to which the *placenta* adheres, or the different situation of the child, may cause great variety here.

The strong outer membrane and cellular one immediately within it, did not seem in any of the pregnant *uteri* I saw dissected, or by the accounts of any authors, to have been much altered by the great distension they underwent.

Heister *, *Nortwyk* †, and Mr. *Hunter*, observed the same muscular appearance under this outer cellular membrane, as I did without any regular layer of fibres; as *Ruyfch* has described and painted ‡: nor could I observe any such regular fibres in a woman who died a few hours after child birth, whose body I dissected while I was in Mr. *LeCat*'s house at *Rouen*, tho' *Nortwyk* || seems to think, that if they are to be seen in any ute-

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* Anat. Compend. Tom. 2. p. 80.

† Hist. Uteri pars 2. § 87. p. 106.

‡ Advers. Dec. iii. Tab. 3. Fig. 1.

|| Hist. Uteri pars 2. § 87. p. 107.

rus, it must be in such a one. Some few fibres there were, that went in an irregular, tho' something circular manner; which, with the help of my dissecting knife, I could have carved into a very pretty muscle. Authors have differed much about this muscle; some affirming that they have seen it, while others have denied that any such thing ever existed. These different opinions are all taken notice of, and quoted, in *Boerhaave's Praelect.* published by *Haller*; Vol. v. § 664. to which book I shall refer you.

I have already mentioned, the womb's being much about the thickness of an unimpregnated one: this however is not always the case; and authors have differed vastly in their opinions in this point: some alledging, that the *uterus* is always thicker; others that it turns thinner; while others have affirmed, that it does not alter in its thickness by impregnation. This great difference of opinions, must have been owing to the different *uteri* which have been examined. As Dr. *Nortwyk* has quoted the passages of different observators concerning this dispute; I shall refer you to his *Historia Uteri* * for their sen-

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* Pars 2. § 80.

timents about this matter ; and only observe, that Dr. *Smellie*, Mr. *Hunter*, Mr. *McKenzie* and others, who practise midwifery here, and have had occasion to see a good number of impregnated wombs, are of opinion, that in general, the *uterus* does not alter much in its thickness by being distended ; tho' sometimes it is found thicker, and sometimes thinner, than ordinary : and in a collection of *uteri* in Dr. *Smellie's* possession, there are wombs which seem to favour all the three different opinions. One of the wombs in this collection, is remarkably thin, not being above the third part so thick as an unimpregnated *uterus* generally is. If I was to form a judgment, from the few *gravid uteri* I have seen, I would be inclined to think, that if the womb alters in its thickness at all, it rather turns thinner ; but the difference is so small, for the most part, that it is difficult to form a judgment about the matter. I ought however to observe, that the *gravid uteri* will be considerably thicker during life, when they are full of blood, than they are in dead bodies, where the vessels are all collapsed. The difference was considerable, in the bulk and thickness of the womb, before we injected the

the veins, from what they were, after we had filled them with tallow.

By some few of the *sinuses* having a little of the red injection in them, it may be conjectured, that some of the small branches of the uterine arteries opened into them: but I could not observe their orifices; and they themselves were so small, that I only saw one or two small twigs running along the membrane, that lined some of the *sinuses*.

SINCE, neither in this subject, the least drop of either the injection that was thrown into the arteries or veins of the mother; nor in another woman, who died when four months gone with child, whose uterine arteries I injected with coloured tallow; any of the injections were found in the secondary vessels, tho' a great deal of injection, in both subjects, was found extravasated between the *placenta* and *uterus*; I must be of opinion, that there is no *anastomosis* between the uterine and secondary vessels; and I could easily see, how the extravasated tallow, injected with force, could make its way into the cellular substance both of the *placenta* and womb.

SUCH serpentine convoluted very small arteries, which Dr. *Albinus* has taken notice of, and

and which I saw likewise, in this subject, on the inner surface of the womb, are not to be observed in any other organ of the body, so far as I remember.

NOTHING remarkable was observed in the membranes of the *ovum*, more than what is mentioned by the accurate anatomists *Albinus* and *Haller*: but Mr. *Hunter* observed a number of veins going from the *uterus* in continued trunks, into the substance of the outer *lamella* of the *chorion*, in a pregnant woman he dissected, and whose veins he had previously injected with yellow wax: these he has delineated in one of his figures, which will soon be published. We could observe none of them in our subject; but then, all the forepart of the *uterus* was separated from the outer *lamella* of the *chorion*, before the veins were injected. Tho' Mr. *Hunter* observed veins, he could not see one artery, continued from the womb to the *chorion*; and the arteries of his *uterus* had been filled with injection, as well as the veins.

IN five of the subjects mentioned by my father, in *Medical Essays* *, two of which I saw, the *placenta* adhered to the forepart of the

*Vol. 2. Art. 9.

the womb ; while in our present subject, and those of *Vater* and *Albinus*, the *placenta* was fixed to the back part of it, and Dr. *Smellie* assures me, he has found it attached to the *os uteri*. I think therefore it is plain, that there is no particular part to which we can say the *placenta* is always fixed : from which it may be justly concluded, that the *chorion* is not all equally capable of being *placenta* ; and that the *placenta* does not owe its existence to the energy of any particular spot of the womb ; but that it is an original part which adheres to whatever place, not only of the womb, but of the Fallopian tubes * or *abdomen*, † it happens to be contiguous to ; where it causes a considerable flow of liquors, by the absorbing powers of its vessels ; in the same way as the sucking of the nipple is the most effectual way of causing a flow of milk to the breasts. Thus the part of the human womb where the *placenta* is applied, has its extreme veins enlarged into *sinuses* : and the smooth surface of the womb and its *cornua* in cows, is raised into glandular-like bodies where each *placenta* is placed.

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* See two examples of this in DE GRAAF de mulier. organis, cap. xiv. quoted from RIOLAN.

† An example of this, in *Hist. de l'Acad. des Sciences* 1716.

THO' the contrivance of the blood returning from the *foetus* being poured into *sinuses*, is a very good precaution against haemorrhagies; yet the veins of the womb that communicate with those *sinuses*, being so large as they are, one would think, that more frequent haemorrhagies might happen at delivery, was it not for the oblique manner in which the most internal coats of the womb are placed by way of valves over the orifices of these *sinuses*; so that whenever the womb collapses or contracts, these orifices are almost intirely covered by these membranes.

THE oblique situation of the *foetus*, the erect, or what is generally called the natural one, in the two I saw formerly, the transverse one of *Nortwyk* and *Cowper*, persuade me, there is no determined situation for a *foetus*, as is commonly thought; but that it may change places in its stirrings, as women generally feel.

IT has long been the received opinion, that *foetuses* are mostly placed in the womb, in what is called the erect or natural situation; that is, with the child's forepart towards the mother's belly; its head uppermost, and reclined

clined forwards and downwards, and that in the last months of pregnancy, or immediately before birth, the head, by its weight, falls down towards the *os uteri*, so that the face of the child is turned towards the *os sacrum* of the mother, and in this manner is delivered. All this account, both of the child's position, and falling down of the head, I must doubt of; for, when I attended courses of midwifery I examined a great number of women, in all the different times of pregnancy, from six to nine months gone with child, and in the greater number of them I felt the head down; and Dr. *Smellie* and Mr. *Hunter* assure me, they have generally observed the same thing: so that this seems to be rather what ought to be called the natural situation. If the child be sometimes in the erect situation, and its head falls down, I cannot think that this change of posture is owing to its greater weight at this, more than at any other time; for, the head of a *foetus* is proportionally larger and heavier, the younger it is: but to the child, thro' a natural instinct, endeavouring to avoid the pressure which its head would suffer by the contraction of the bottom of the womb, and the detrusion
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of the *diaphragm* during labour-pains, and that, by being turned, its feet may act with advantage, by pushing against the *fundus uteri*, to assist the delivery.

THE enlargement of the veins of the womb during pregnancy, is really surprising, as may be judged by observing their transverse sections in their collapsed state, as represented in the cut edges of the womb; see TAB. iv. and v. and by the size of the spermatic veins, in TAB. vii. *fig.* 5. I ought however to observe, that the trunk of the vein is here represented less than it should have been; for when the womb was cut out of the body, some of the injection was, by accident, pushed out of the veins, before the figure was made. The situation of this vein and artery, may let us see, how easily *Arantius* * and other anatomists, before the injecting art was found out, might imagine the artery entered into the vein; that the arterious and venous blood might be blended together before they reached the womb. Injections plainly shew, there is no such uncommon *anastomosis* between the vein and artery.

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* ARANTIUS de foetu, cap. iii. p. 7.

THE womb, by being distended, presses on the iliac veins, and makes a difficulty to the blood returning from the lower extremities, and even to the blood returning by the branches of the iliac, from the womb itself, long before the *uterus* has rose so high as to press upon the spermatic vein : is not then this difficulty of the blood's returning by the iliacs, the cause of the vast distension of the spermatic veins during gravitation, as well as it is often the cause of the oedematous legs, and swelled *labia pudendorum*, at that time?

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A R T.

ART. XVIII.

Additional Observations on Gravid Uteri, by
ALEXANDER MONRO, *Student of Medicine*
in the University of Edinburgh.

GENTLEMEN,

HAVING wrote to my brother a few observations which I had made in examining a *gravid uterus*; he desires me to add such of them, as I thought might be a proper supplement to the paper which he sent you lately on this subject.

THE woman I dissected was about forty years of age, had born four children, and was said to be five months gone with child; her *uterus* being almost as large as in my brother's subject, tho' more of an oval shape, with the forepart of it contiguous to the *peritoneum*.

HAVING observed so far hastily, I was obliged to remove it from the body.

THE *os tincae*, then seen from the *vagina*, was considerably larger, and the *cervix uteri*

was

was distended lower down than is represented in your TAB. v. *fig.* 3. which possibly might be owing to this woman's weakness, and to the number of children which she had born.

A liquor, of a thicker consistence than the oil of turpentine and vermillion which my brother made use of, being injected into the *hypogastric* arteries, filled the spermatics, and a considerable artery in the *round ligament*. The joining of the trunks of the two former upon the sides of the *uterus* was so large, that it was impossible to say, from which of them the branches distributed to its substance were derived. The right spermatic was small, but the left was as large as a goose quill; the hypogastrics being of a middle size betwixt the two.—By blowing air into any of the veins, we had sufficient proof of their monstrous size, and of their numerous and very large communications; the whole *uterus* being at once inflated.—Corresponding to the artery before mentioned in the *round ligament*, I observed a large vein in it, which probably discharged itself into the external iliac.

WHEN the *uterus* was cut open upon its fore part, near three fourths of its thickness had plainly the appearance of a muscle; the fibres

fibres being of a pale red or flesh-colour: and when part of it was boiled in water, we could not have distinguished it from any other piece of boiled flesh; most of its fibres then seeming to run obliquely from the *cervix* towards the *fundus*; others appearing transverse. To which if we add the known sensibility of the womb, and its strong and sudden contraction at birth, we have enumerated all the essentials required in the constitution of a muscle.

As the internal cellular, or rather succulent fungous membrane of the womb, especially where the *placenta* is fixed, is of a considerable thickness; I cannot conceive, how any thing like to an orbicular or radiated muscle could ever be alledged to be seen upon its inner side, without dissection.

THE *placenta* and *foetus* were situated nearly in the same manner, as is represented by *Bidloo*; a small sketch of whose figure is in your TAB. vi. *fig. 2.*: tho' possibly the situation of the *foetus* might have been altered, by our having carried the *uterus* above half a mile before we examined it.

THE *liquor amnii* was of a clear brown colour; upon adding oil of vitriol, or ol. tart.

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p. d. to it, no remarkable change could be observed: which proves that it contains little ferous or urinous; for the *oil of vitriol* would have coagulated the former, and the latter would have betrayed itself upon the addition of the fixed alkali.—When it was brought to a boiling heat, it threw up a thin whitish scum; and an ounce of it boiled dry, left about a scruple of what appeared to be a ferous *coagulum*.

WE could not observe any thing like an *allantois*: and, when the *urachus* was afterwards examined, it neither allowed air, water, or quick-silver, to enter it from the bladder; tho' I have seen it pervious for an inch or more in children, born at the full time.

THE *cervix uteri* being now cut open; the whole inner side of it was covered over, especially near to the *os tincae*, with clusters of vesicles, some of which were of a considerable size, filled with a dark brown-coloured mucus; and small orifices, containing the same kind of liquor, appeared in their interstices.

WHEN the *placenta* was taken off, the injection, which had been thrown into the hypogastric arteries, was found extravasated between
twixt

twixt it and the *uterus*; many lumps of the wax being, as it were, wedged into the substance of the *placenta*, without having entered any of its vessels.—We then saw many small serpentine arteries with their orifices opening upon the internal membrane of the *uterus*; and other, generally larger, convoluted arteries, whose extremities we could not as yet observe, appearing prominent in the interstices of cavities filled with the wax; and which, in the foregoing paper, are mentioned under the name of *sinuses*: the real structure and situation of which, being little understood, tho' often talked of, I shall relate all that I observed in this subject; and then, by comparing it with the common description, we may better understand what has misled most anatomists.

THE *sinuses* are seen without any previous dissection upon the inner side of the *uterus*, chiefly where the *placenta* has been fixed; for there are but few of them, and these very small, in any other part of it.—Their sides are membranous, that, next to the cavity of the womb, being in each very thin, with a large orifice in it.—They can be readily distended, by blowing air, or injecting a liquor

quor into the veins ; or they may be filled from the arteries, tho' with much greater difficulty, as happened in our injection: when stretched, they appear of a spheroidal shape ; and, the diameter of their orifices being less than that of their cavities, we may give them the name of *sinuses*.—Upon dissection, we observe arteries opening, at least into some of them, as I shall afterwards prove, and considerable veins are continued from all of them ; the veins and *sinuses* having all large communications with each other ; and, tho' some of the sinuses may have only two or three small veins opening into them, yet, in general, the diameter of the veins continued from them, is not greatly inferior to that of the sinus.

SINCE, from this description, it appears, that these cavities are only the somewhat dilated extremities of the branches of the veins ; we may account for their formation, from the more than ordinary difficulty there is in the return of the blood from the *gravid uterus* ; which being therefore accumulated, must stretch all the veins greatly, but such parts of them most as resist the least : consequently their extremities which are only covered

vered by the lax internal cellular membrane of the womb, will be most dilated, or put on the appearance of sinuses; and this effect will be most conspicuous where the greatest flow of liquors is, that is, at the place where the *placenta* is attached to the womb; as my brother has justly remarked.

IF we compare the above description with the common notion of a sinus, we shall find they differ widely.—They are generally defined to be large cavities in the middle of the substance or fleshy part of the womb, that have small branches of arteries and veins opening into them, with canals, whose diameter is considerably less than that of the sinus, running obliquely thro' the substance of the womb to open upon its inner side.—But since, after a diligent search, no sinuses of this form could be seen in this subject; as neither my brother nor the accurate *Albinus* paint or describe any such; and, as the ingenious and diligent Dr. *Haller* positively affirms, in one of his latest works*, that after repeated experiments, he could not observe them; it seems probable, that the trunks of the veins have been mistaken for sinuses; and their branches, for canals opening
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* Prim. Lin. phys. § 804.

into the cavity of the womb; nay, a late learned author applies these names of *sinuses* and canals, to the trunks and branches of the veins painted in *Albinus's* 7th table of the *gravid uterus*.

THE existence of *sinuses* and canals being then imaginary, and the cavities which we have described under the name of *sinuses* being entirely the creatures of impregnation, and not to be found in the *uterus virgineus*; or being at any rate, from the size of their orifices, unfit receptacles of the blood; we cannot surely account for the *menstrual flux* or any other phænomenon from such a structure.

As the *sinuses* were filled by our coarse injection, thrown into the arteries, I imagined, that, by a careful dissection, the openings of the arteries into them might possibly be observed; but, I found more difficulty than I expected, from the size and very large communications of most of the *sinuses* and veins. Near to the edges of the *placenta*, where they were not so frequent, I was lucky enough to discover, with certainty, several orifices of arteries, some of which were of a considerable diameter, opening directly into the *sinuses*: and since none of the anatomists

have painted or described these openings, tho', in the imaginary structure of the *sinuses*, they are supposed; I have caused a few of the most distinct to be represented in TAB. 7. *fig. 7.* where *A* represents the inner surface of the womb, to which the edge of the *placenta* had been fixed;—*bb*, two small arteries appearing after having pierced the muscular substance;—*ccd*, their serpentine and spiral turns;—*fs*, their openings into the *sinuses*; which were plain beyond dispute, when the wax was taken out of the *sinus*, and the artery gently pressed, the *sinus* being again in part filled from it, as is represented at *f. w.* Nay, it is evident, that the arteries had communications with the *sinuses* also in my brother's subject; since the *placenta* was covered over with the extravasated oil of turpentine and vermilion, which was injected into the *aorta descendens*; and which therefore seems to have filled the *sinuses*, but to have been thrust out of them by the coarse green injection afterwards thrown into the veins.—*ee*, small arteries which were observed every where opening upon the internal membrane of the womb;—*ff*, others which seem to have been torn in separating the *placenta*.—

And

And, in all that part of the womb to which the *placenta* had been fixed, when the cellular membranes and extremities of the veins were dissected off; innumerable arteries, of all sizes, from that of a crow-quill downwards, convoluted in a most surprising manner, were observed; which were nothing so conspicuous in any other part of the womb, treated in the same manner.

A R T.

ART. XIX.

*Nelgic is. Of the Difference between Respiration and the
 Uthyerogte Motion of the Heart, in sleeping and wa-
 Verhandelt. king Persons, by ROBERT WHYTT, M. D.
 1. Dec. p. 313. &c.*

OF all the parts of the human body, there is none whose structure is more subtile, and whose several functions and uses are less known, than those of the brain. It is little to be wondered at, therefore, if authors have failed of giving a satisfactory account of *sleep*, which is one particular state of this unknown organ.—We have elsewhere offered our conjectures concerning the reason why the vital motions continue in time of sleep; when the organs of sense become less fit to receive the impressions of external objects; and when the muscles of voluntary motion are more relaxed and remain at rest*. But, as respiration and the motion of the heart,

* Essay on the Vital and other Involuntary motions of animals, sect. xii.

heart, suffer some change in time of sleep, *i. e.* become then slower, fuller and more equable, than when we are awake *, it may be worth while to enquire a little into the reason of this *phaenomenon*.

It has been shewn, that as the dilatation of the ventricles of the heart, is owing to the force of the reflux venous blood; so their contraction is produced, by the same blood acting upon them as a *stimulus* †: and that the heart can only be affected by *stimuli*, in so far as it is a sentient organ, *i. e.* endowed with feeling ‡. Whence it must follow, that the slowness of the pulse in sleep, and indeed in every other case, can only arise from one or more of the following general causes. *viz.* 1. A diminution of the stimulating quality of the blood. 2. Its slower return to the heart; or, 3. A less degree of sensibility or aptitude for motion in the heart itself.

* BOERHAAVE Institut. Med. § 599.

† Vid. Essay on Vital and other Involuntary motions of animals, sect. iii. and iv.

‡ Ibid. sect. x. p. 271. &c.

I. WHEN one has eat or drunk a great deal before sleep, his pulse will be, not slow, but quick and full; because the stimulating power of the blood is increased, by a large quantity of chyle received into it. Much the same thing may happen from sleeping in too hot an air, or under too great a weight of cloaths; for we know, that heat quickens the circulation of the fluids in all animals. On the other hand, when one has fasted long before sleep, and lies very cool, his pulse will, in time of it, be unusually slow.

BUT when the blood is neither loaded with new chyle, nor altogether destitute of it; neither too much heated by cloaths or the external air, nor too cool, thro' want of proper covering; its stimulating quality will neither be augmented nor diminished by sleep; but will continue the same as in a person who is awake in the same circumstances. It remains therefore, that the slowness of the heart's motion in time of sleep, be owing, either to the slower return of the venous blood to it, or to some diminution of its sensibility.

II. EVERY one knows, that the affections of the mind disturb the motion of the heart;
that

that the pulse is quicker when we sit or stand than when we lye; and that the action of the muscles of voluntary motion, not only promotes the return of the blood to the heart, but determines it thither, with much greater force than usual. In sleep therefore, where the horizontal posture of the body, the quiescence of the voluntary muscles, and composure of the mind, all concur to render the return of the venous blood to the heart, more equable and slow; the contraction of this muscle must be renewed at greater intervals, and with more regularity, than when we are awake, and the circulation is quickened or disturbed by some, or all, of the above-mentioned causes. But, if no farther circumstance, tending to retard the heart's motion, were found in sleep, the pulse should be equally slow and full in a waking person lying at rest in a horizontal posture, and whose mind is composed, as in the same person in time of sleep; which, however, does not seem to be entirely the case: for tho' the difference may be small, and there may be many causes which may render it difficult, by observation, to determine that difference with any degree of certainty; yet the remarkable
slowness

slowness and fulness of the pulse in the deep sleep, accompanying an apoplexy or occasioned by *opium*, makes it highly probable, that even in the much gentler sleep of persons in health, the pulse is somewhat slower and fuller, than it would be, merely from the composure of mind, horizontal position of the body, and quiescence of the muscles of voluntary motion. Let us therefore see, whether the slowness and fulness of the heart's motion in time of sleep, may not be in part owing, to some diminution of the sensibility of this organ.

3. In time of sleep, as the exercise of the several senses, is either suspended or much impaired; so the sensibility or feeling, with which the organs of the body are more or less endued, seems to be rendered less acute. Thus we feel ourselves affected with a kind of *stupor*, when we are just falling asleep, and are then insensible of lesser *stimuli*. The thin rheum, which, by irritating the wind-pipe, keeps us almost perpetually coughing when awake, gives us little or no disturbance in sleep: any extraordinary *stimulus* in the guts is also less perceived then; and hence it is, that a dose of any purgative taken at night, is
much

much longer before it operates, than when it is swallowed in the morning. If the heart therefore, like the other organs of the body, becomes less sensible or irritable in time of sleep, it will not be so quickly excited into contraction as usual, by the venous blood rushing into its cavities ; and hence its contractions will not only be more slowly repeated, but the pulse will be full, because the ventricles do not contract, till they are much distended with blood. This will still further appear, if we consider how remarkably slow and full the pulse is, in an apoplexy, where the sensibility of all the parts and their aptitude for motion, are much more impaired than in common sleep: and how *opium*, which occasions sleep, and lessens the sense of feeling every where thro' the body, when given in a large dose, renders the pulse uncommonly slow and full. Thus the heart of a frog, into whose stomach and guts I had, an hour before, injected a solution of *opium*, was observed to beat near four times slower than usual ; and the auricle and great veins leading to the heart, were remarkably distended with blood, as was also the ventricle of the heart before every contraction. At

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the same time the other muscles of this animal were so intirely deprived of feeling as not to be, in any degree, affected by pricking or tearing their fibres *.

DOES not the slower digestion of the aliment in time of sleep, proceed, partly, from the peristaltic motion of the stomach and guts being then repeated after longer intervals? In dogs who have got a large dose of *opium*, this motion is very much lessened or totally suspended, the food last received into the stomach remains there indigested, the guts are more than usualy empty, and the lacteal vessels invisible †. In sleep, therefore, not only the heart but the stomach and guts also, become less sensible of the *Stimulus* usualy affecting them, and consequently repeat their contractions more slowly.

De Gorter, differing from *Boerhaave* and other authors, thinks, that the pulse must be, not fuller, but softer in time of sleep, because the circulation of the blood is allowed, then, to proceed more slowly than when we are awake.

* Essay on the Vital and Involuntary motions, &c. p. 372.

† *KAUU impet. faciens HIPPOCRAT. dictum*, No 434.
435.

wake †. But, from what has been said, it appears, that the fulness of the pulse in sleep, is not owing to the quicker circulation of the blood, but to a less degree of sensibility in the heart, whereby its ventricles are not excited into contraction, till they have been more fully dilated, then usual, by the returning blood. It is, however, to be observed, that the fulness of the pulse in sleep, may be owing partly, to the fluids passing, with greater difficulty, thro' the very small lateral arteries, and the secretory tubes of the glands *. For we know that the fulness or softness of the pulse does not depend, solely, upon the quantity of blood thrown out by the left ventricle of the heart, but also upon the more or less difficult passage of this fluid thro' the extreme arteries; since, in proportion as these are obstructed or open, a greater or less resistance will be opposed to the blood projected by the heart.

AFTER what has been said of the slowness of the heart's motion in sleep, it will be easy to shew why respiration should be performed, then, at greater intervals.

THE

* BOERHAAVE Institut. med. § 597.

† Exercit. de somno et vigilia. § xl.

THE cause exciting the alternate contraction of the inspiratory muscles, is an uneasy sensation in the lungs, occasioned by the blood pushed into their vessels by the right ventricle of the heart*. If then less blood is sent, in a given time, into the lungs, in sleep, than when we are awake; the necessity of new supplies of fresh air will be lessened, and consequently inspiration will be performed at greater intervals.

FURTHER, as in time of sleep, the sensibility of the lungs, like that of the heart and guts, must be somewhat impaired, respiration must also, on this account, be performed more slowly; for the inspiratory muscles will not be excited into action till a greater degree of irritation, than usual, be occasioned by the blood accumulated in the pulmonary vessels. And, to this it is owing, that respiration is not only slower but somewhat deeper in time of sleep, than in a waking person at rest in a horrizontal position.

IN comatous and apoplectic cases, where all the feelings of the body are much more impaired than in ordinary sleep, respiration is not only much slower and deeper than usual, but,

* Essay on Vital and Involuntary motions, &c. p. 176. &c.

but, sometimes, after expiration is finished, a pause of 15, 20, 30, or more seconds will intervene, before a new inspiration is begun. Much the same thing happens to animals who have swallowed too great a quantity of *opium* *.

Now, if it be reasonable to ascribe the slow, deep and interrupted breathing, in such cases, to the insensibility which attends those diseases of the head; and which *opium* never fails to produce, when taken too liberally; are we not hence led to conclude, the less remarkable change of breathing which happens in sleep, to be owing, partly, to the sense of feeling in the lungs, being then somewhat diminished, tho' in a much less degree than in those morbid cases?

To conclude with summing up what has been said in a few words; in ordinary sleep the sensibility of the heart and lungs suffer so small a diminution, that their motions will be very little more affected by it, than they would be from the horizontal position and rest of the body, and composure of mind attending it. In the deeper sleep, which succeeds great fatigue, the motions of the heart

* Essay on the Vital and Involuntary motions, &c. p. 194.

heart and lungs will be more observably altered. And, in the most profound sleep, occasioned by *opium* or a morbid state of the brain, where a general insensibility reigns over the whole body; the pulse will become much more remarkably slow and full, and respiration slower and deeper.

A R T.

ART. XX.

Remarks on the Intercostal Muscles, by ALEXANDER MONRO, F. R. S. Professor of Anatomy in the University of Edinburgh.

TAUVR^{*}, shewed the misapplication of Mr. Bayle's proposed demonstration of the action of the internal intercostal muscles, from his inattention to the ribs not keeping in the same parallelism when they are raised, as when they are depressed, because of the greater motion of the lower than of the higher ribs; and he took notice of the same demonstration, proving equally that the anterior part of these internal intercostals must act in inspiration, as Bayle would have their posterior part to act in expiration: but neither he, nor any other writer of my acquaintance, has applied this demonstration of Bayle's, which, granting the parallelism of the ribs to remain the same, is a good one, to account for the defect of the internal

* *Anat. raisonn. part. 2. chap. 14.*

nal intercostals between the spine and the angle of the ribs, and for the deficiency of the external intercostals from where the ribs begin to turn upwards to the *sternum*; a fact which anatomists are now all agreed in. The application of the demonstration to this purpose, is the intention of this paper.

LET AB (*fig. 1. TAB. viii.*) be the spine, CD the *sternum*, EFGH, and IKLM two ribs; then EK and GM are lines in the direction of the external intercostal muscles, and FI, and HL are in the direction of the internal intercostals.—Now, says *Bayle*, if both ribs rise, the angle FEI must be enlarged, and its hypoteneuse FI, the fibres of the internal intercostal, must be longer: whereas muscles generally are shortened when they act.—Near to the spine, where the ribs cannot be brought nearer at one time than another, this reasoning is just; and therefore the internal intercostals are not placed nearer to the spine than where the motion of the ribs is large enough to allow the lower rib to approach so much to the one above it as to do more than compensate for the elongation of the hypoteneuse of the angle FEI, *i. e.* the internal intercostal is only to be found

found farther foreward than the angles of the ribs.

NEAR to the *sternum*, the cartilages cannot be brought nearer; and therefore, when they are raised up, HL, the fibres of the internal intercostal may be shortened, because the angle HML is made less: but, if there were fibres of the external intercostal here, as GM, they would be made longer, because the angle GHM is enlarged by raising the ribs; and therefore no external intercostal muscles are placed at this forepart of the *thorax*.

L 11

A R T.

ART. XXI.

The Cure of a fractured Tendo Achillis; by the same.

OUR predecessors were either not subject to the breaking or tearing the *tendo Achillis*, or it was mistaken for a sprain, or some such other disease by physicians and surgeons; for it is rarely taken notice of in books; and the oldest and best practisers here assure me, they had not observed it or heard of it, till between twenty and thirty years ago; since which this accident has happened to a considerable number of people of this country, of whom I know sixteen, and several of those have broke the tendons of both legs; among the rest, I have been a sufferer this way; and, upon a comparison with my brethren in affliction, my cure is the most compleat of any I know; there being neither pain, stiffness, weakness nor observable shriveling in my left leg where the *tendo Achillis* was broken: whereas most of the other gentlemen have some of those uneasinesses,

easinesses, and several have all of them. On this account, I think it my duty, for the benefit of future patients, to make my method of management as public as I can; which probably will be best done, by communicating it to your society.

When my tendon was torn, it cracked as loud as if I had suddenly broke a nut under my heel; which the company believed had happened: and I had such a sensation as made me imagine that the heel of my shoe had struck a hole in the floor; which is the feeling that several have had as well as I: while others complain as if they had received a smart stroke with a stone or cane on the part.— I suspected immediately what the case was; and therefore, after feeling where the hollow was, between the ends of the broken tendon, I took the left foot in my right hand, with which I extended the foot strongly; and, as soon as I was put in a seat, pressed down the calf of my leg with my other hand: in which posture I kept them till Mess. *John Douglas* and *James Russel* surgeons of this place came; and, after being satisfied of the rupture, by pressing their fingers into the hollow between the two ends
of

the tendon, they applied compresses, and a bended board on the upper part of my foot and fore part of my leg, which they kept, as near as they could, in a straight line, by a tight bandage made with a long roller.—This dressing became soon too uneasy for me to bear, and the board was liable to shuffle whatever care was taken in the application of the bandage: therefore I changed it for the following compound one, consisting of two pieces. (See TAB. viii.)

Fig. 2. is a foot-sock or slipper A, of double quilted tiking; from the heel of which B, the quilted strap C, is of such a length as to come up above the calf.

Fig. 3. A strong quilted calf-piece L, with pye-holes MM on each side, through which a lace, *fig. 4.* is to be passed; and, with a buckle N, so placed on its back-part, that, when the lacing was on the outside of the leg, the buckle was in the middle of the lower part. Two rows of pyeholes are here represented on one side, either of which is to be used according to the thickness of the leg.

HAVING

HAVING then wrapped soft flannel, smoaked with the fumes of benzoin, round my foot and leg ; I put on, as in *fig. 5.* the foot-sock A, and calf-piece L, and bringing the strap C, thro' the buckle N, I could by it extend the foot, and pull down the calf to what degree I thought fit, and there secure it with the buckle.

THIS bandage answering my intention quite well, I wore it night and day, drawing the strap tighter when I inclined to sleep, and relaxing it when I was fully awake and on my guard ; at which time, likeways, I placed my foot on a stool, in the posture represented at S, and frequently shifted the calf-piece or made the lacing of it looser, to prevent the swelling of my foot, which it was in danger of occasioning, when it became too tight, by being drawn down by the strap.—After a day or two, I found my toes uneasy, from the foot-sock pressing them ; and therefore I undid the sowing of it at the toes, from *d*, to *d* ; and have caused it always since to be made open there, for others who used it.

DURING two weeks, I made no motion nor effort with my foot ; but was carried on a chair, running on castors, from one part
of

of my house to another. After this, I began to move the foot backwards and forewards so gently as not to give pain, and gradually increased those motions; but always stopped the flexion of the foot, or extension of the leg whenever I felt the least uneasiness: frequently I continued these motions for half an hour together, with the diseased limb; but kept the other as much at rest as I could.

WHEN I began to walk, I always put the left leg some way before the right, that the left foot might be well extended; making use of a cane in my right hand, to prevent any danger of falling.

THE void between the two ends of the divided tendon, became insensible in few days, except that a softness was felt there, more than any where else; but this part turned gradually thicker and harder, till a knot was formed in it, which one, in feeling thro' the teguments, would have thought to be a piece of cartilage, as large as a middle sized plumb, which has greatly decreased, and become softer, for some years past; so that, at present, it is much less perceptible than formerly.

SOME

SOME weeks after the accident, I began to pour cold water on my leg and foot, causing them to be well rubbed immediately after; but the water, instead of strengthening the member, as I expected it would have done, made it cold and weak; for which reason I soon forebore the use of it, and caused the leg to be rubbed twice a-day strongly, with *unguentum altheae*, or some such greasy stuff, to protect the skin from excoriation by the friction. This *manoeuvre* was continued till I began to employ the limb freely.

BEING obliged to go abroad after six weeks, I put on a pair of shoes with heels about two inches high, and applied the machine, which I am just now to describe, in the day time, instead of the former bandage; which, however, was always put on at night, for a month more.

THE new machine, *fig. 6.* was a piece of steel, the middle stalk of which A, is narrow but strong: the ends BB, are then broad and concave, adapted to the convexity of the foot and forepart of the leg. Three staples CCC, stand up from the forepart of the steel, one being in the middle of each of the broad ends, and the third in the middle of the stalk.

stalk. All the steel except the stapples was covered with chammoy leather, and the concavities of BB, were well buffed, as the softer rupture bands commonly are.

AFTER I had put on my shoes and stockings, one end of this machine was put on the broad of my foot, nearer the toes than the buckle of the shoe; and the other end was placed on the fore part of the leg: then one ribband, or a thong of leather, *fig.* 7, 8. was put round the foot, and another round the leg, to pass thro' the two stapples near the ends of the machine, and there secured with straps or buckles, but without being drawn tight.—A third strap or ribband, *fig.* 9. having its middle D, applied in the hollow of the foot, immediately before the heel had its ends passed on each side of the foot, thro' a *sinus* or noose EE, of a fourth thong of leather F, that came round the quarter-heel of the shoe, to be afterwards put thro' the middle stapple; where, after these ends GG were drawn as tight as was thought convenient for extending the foot, they were secured with the buckle, or with knots. See the application in *fig.* 10.—I wore this machine always in the day time, for five months: tho',

Fig. 2.

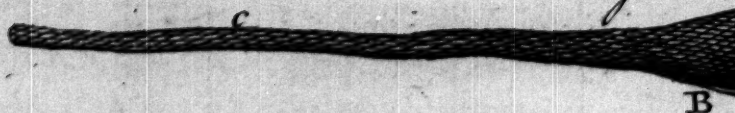


Fig. 4.



Fig. 5.

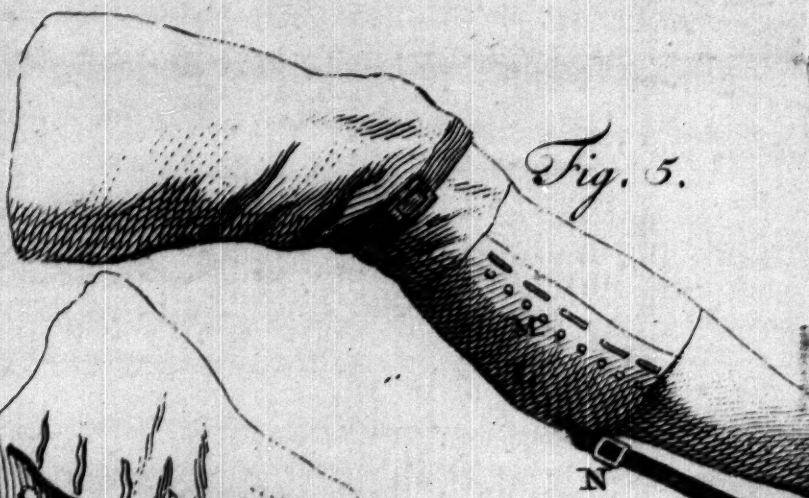


Fig. 10.

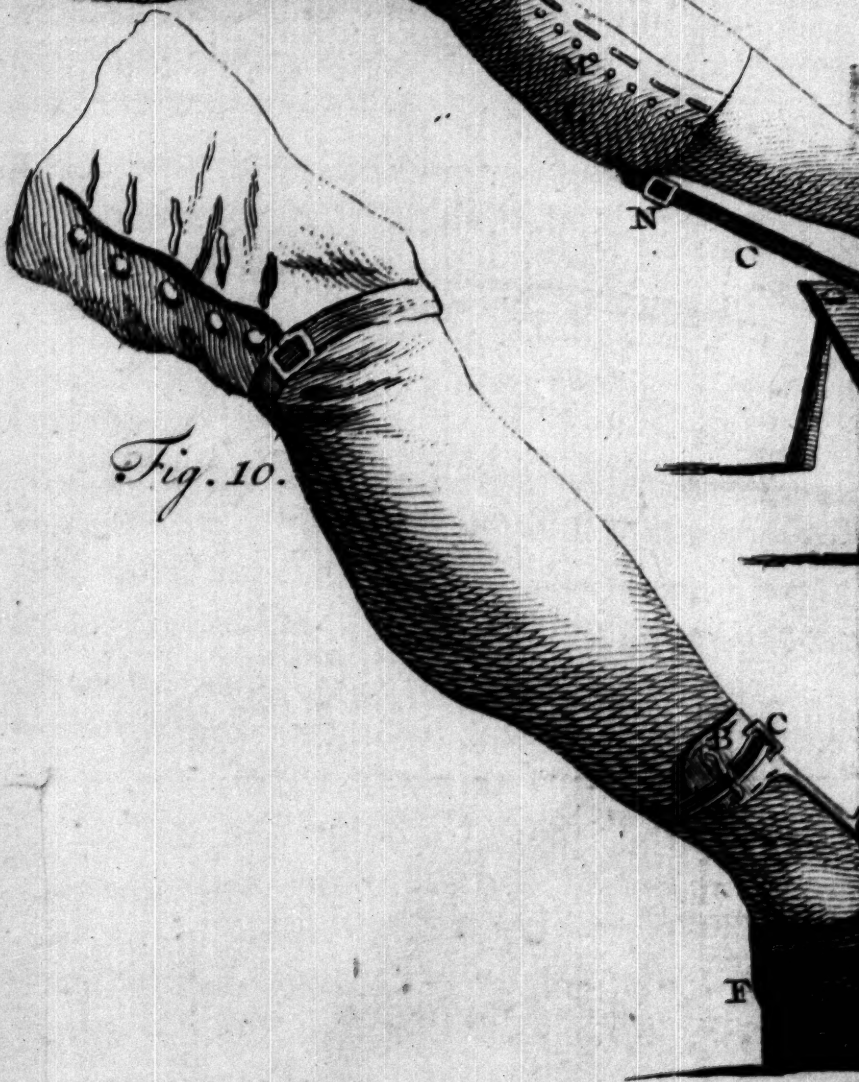
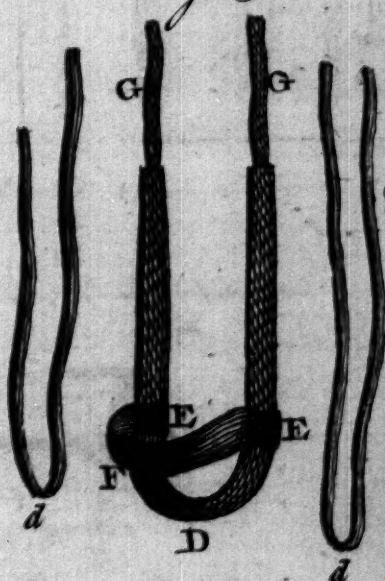
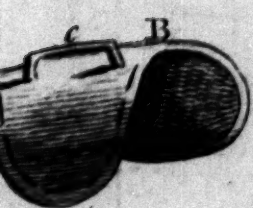
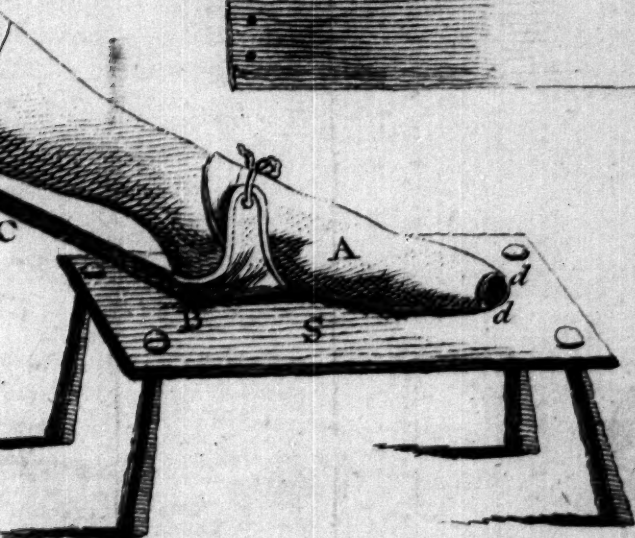
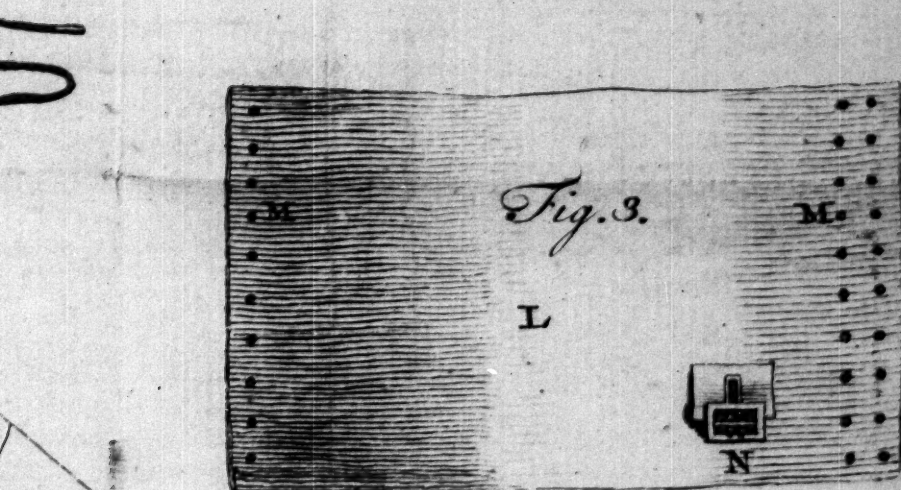
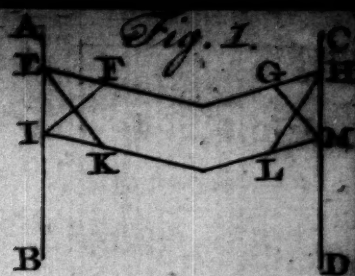
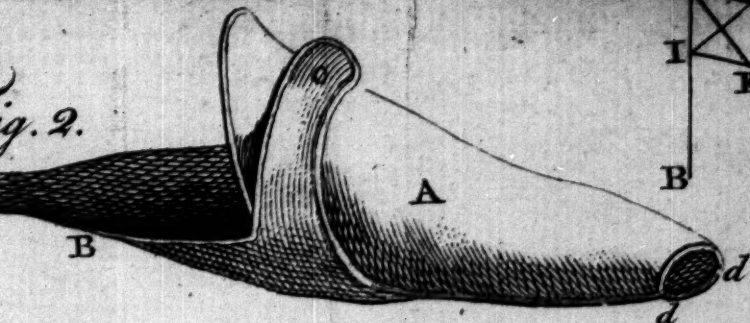


Fig. 6.





as it is inconvenient, by shuffling out of its place sometimes ; I think a thong of leather sowed at one end to the upper middle part of the quarter-heel of the shoe, and fastened at the other end to a garter, put above the calf of the leg, would serve instead of it without inconvenience.—All that time I never walked the streets, but was carried in a chair.—In going down stairs, I always put the diseased leg first down at each step ; and, in coming up, I put the sound leg foremost, by which I shunned the stretching and rearing of the new souldered sinew, which, I knew from what others had suffered, might have made the case worse than it was at first.—The habit I had got of walking after this manner in stairs, became so much a second nature, that I had afterwards difficulty to learn to go up and down in the ordinary way, with the feet alternately.

I continued the high-heeled shoes for two years, causing my boots to be made after the same fashion, when I began to ride ; but have gradually since retrenched their height, till now I wear them of the common make.

DURING all that time too, I mounted my horse at the right side instead of the left one,

M m m

that

that I might shun the rearing my whole weight, by the force of the weakened *gastrocnemei* muscles; and to prevent the accident of straining them, in case the horse stumbled, I kept the left foot deep in the stirrup. In walking up hill, I put that foot a-cross; and, in short, guarded against every other stretching step or motion.

ON comparing the size of the calfs of the two legs at present, the left one is a little smaller than the right one, especially in the morning; but so little, that it is scarce perceptible: thro' the day, the difference becomes less.—The *tendo Achillis* that was broken, is, as most other tendons cured after breaking, considerably thicker and harder than the other; but one must be told it is so, before he can discover it by the eye.

A R T.

A R T. XXII.

An Account of the Disease called Mill-Reek by the Miners at Leadhills, in a Letter from Mr. JAMES WILSON, Surgeon at Durrisdeer, to ALEXANDER MONRO, P. A.

IN obedience to your desire, I send what I have observed concerning the disease, which the people at *Leadhills* call the *mill-reek*, and which all the inhabitants there are subject to; but it mostly seizes, and violently affects the men whose daily business it is to melt down the lead. The melting-houses, where this is done, are called *mills*; because the bellows there are worked by water-mills.

In the slighter stage of this disease, an uneasiness and weight is found about the stomach, especially near the *cartilago ensiformis*; and sometimes it appears like a colic in the intestines. The spittle of the sick is sweat, and something of a blueish colour, resembling what one observes when he chews lead.—The pulse is a little low; the skin is all over cold

cold ; and a clammy sweat frequently breaks out.—The legs become feeble with a prickling numbness ; and there is a debility and laziness in all the body.—The appetite goes away, and they don't digest what food they take.—Sometimes a *diarrhœa* makes a cure ; but, if it continues too long, it is very hurtful.—In this *stadium* the sick are yet able to go about and to work.

BUT, if these symptoms continue long, and spirituous liquors are drunk with an empty stomach, or after working lead ; the disease comes to its second stage : in which, to the former complaints, are added a fixed pain in the stomach and guts, especially in the lower part of the abdomen, extending itself from the one *os ilium* to the other.—The patients become very costive, with the sense of somewhat gnawing their intestines ; and the pulse turns quick with heat on the skin.—Giddiness, with vehement pain, seizes the head ; which is succeeded by an insensibility and *delirium*, like madness of the worst kind ; in so much that they tear their own flesh, and bite their hands ; the extremities tremble, and are convulsed : at last they fall low,

low, the pulse intermits at every 3d or 4th stroke, and they die in a *coma* or apoplexy.

THE reek or smoak rising from the melting lead, is believed to be the cause of this disease; because the melters, who are most exposed to the smoak which comes out often full in their faces, are most subject to this disease, the *mill-reek*.—The people here say they have seen birds, in a calm moist day, attempting to fly thro' the smoak of such a chimney, fall down dead.—Cattle, which pasture near to mills, are often killed; and therefore shepherds take great care to keep their sheep at a distance; which, if not by the smoak, must be hurt by the grass, which I often see made blue by the smoak falling on it. And other animals suffer from the water impregnated with the fumes, or with the lead washed in it.

THE symptoms in them are very like to what men suffer. Dogs, in the last stage, lie either dull and stupid, or bite and snatch at every creature that comes near them: nay, they will gnaw and tear up the ground on which they lie, after they cannot raise themselves on their legs.

THE

THE people at *Leadbills* are so averse to opening of dead bodies, that I could never prevail upon them to allow me to open any who died of this disease. But, in a dog who had it, I found *sludge* * lying on the inner coat of the stomach and intestines; and, in several parts, it was turned to a crust.—The guts were much inflamed in some places, and in others a mortification was begun with holes thro' them.—His *fæces* were very hard; and, where they were of little quantity, the coats of the guts were thick, and the passage thro' them less.

IF proper medicines are given in the first stage of this disease, the patient generally recovers.—If it goes on till giddiness begins, the success is doubtful; and when the cure is delayed to be attempted a little longer, the disease almost constantly proves mortal.

IF the work-people at *Leadbills* would use the following precautions, they might save themselves from this disease, at least would have it very mild.

I. No man ought to go to work fasting; and he ought to take oily or fat food: the *English* mill-

* The fine particles of lead which subside slowly in water, in which lead has been washed.

mill-men on this account hold much better out than our countrymen. A glass of sweet oil pure, or mixed with a little *aqua vitæ*, would be a good morning draught.

II. **PHYSIC** should be taken Spring and Harvest, and whenever any effects of the reek are felt.

III. **ARDENT** spirits ought to be drunk very sparingly ; and ought never to be taken in time of work at the mill, or immediately after it. They increase and fix the bad effects of the leady smoak.

IV. No mill-man, when heated by work, ought to go into cold air ; but to put on his cloaths immediately, and return to his lodging, to change his working cloaths for others, and cool gradually: by which he would prevent catching cold. In this article they are very careless.

V. **IMMEDIATELY** after coming from work, the aliment should be mostly liquid, as broths.

VI. Low and poor diet makes them more liable to be affected, and less able to undergo a cure : these workmen ought to feed on good meat.

VII.

VII. WHEN their business can allow, they should go out of the reach of the reek, to breathe an untainted air, and to take victuals free from lead. But I must caution the labourers at *Leadhills* not to take long journeys: they are more hurt by travelling one day than by working two.

THE cure of this disease depends principally on cleansing the *primæ viæ*: and therefore, after bleeding such patients, if they are plethoric, I give them a vomit of *emetic wine* or *tartar*; which must be a dose double to what would vomit another person; otherways it would have no effect, especially when the disease is violent or has continued long. My father, who has had long practice with such patients under the mill-reek, has always finely powdered *vitrum antimonii* ready; of which he gives half a drachm for a dose; and during its operation, warm water is to be drunk plentifully. If the vomit operates well, and purges briskly too, the patient is in a fair way of recovering; which a second, but a milder dose of *ipecacuanna* with some *tartar emetic* mixed, often makes compleat: but, if the emetic neither vomits nor purges, the patient is generally worse for it; and a stronger dose should

should be given soon.—If it vomits but does not purge, a cathartic of the antimonial kind, or of jallap and mercury, in greater than ordinary quantities, ought to be given: and during the time of purging by the emetic or cathartic medicine, the patient ought to drink warm broth plentifully.—The vomits and purgatives ought to be repeated at proper intervals, till the uneasiness in the stomach and guts, from the disease, is gone.—If these medicines over-do, an opiate may be given at night; but this is to be administered sparingly, lest it bring or increase costiveness, which is the worst thing can befall the patient. At the same time, emollient, anodyne, and laxative clysters, are frequently to be injected for emptying the guts, if the purgatives do not their duty.

WHEN blood or matter are passed with the *faeces*, the emetics and purgatives are to be abstained from, till, by healing, balsamic, but laxative clysters, and mild food, this appearance ceases.

WHEN the belly is much swelled, emollient fomentations should be often applied to it; but, if the madness is begun, little else can be done, than to endeavour to keep the

N n n patient

patient quiet during the little time he has to live.

SOMETIMES it happens, that, after the complaints of stomach and guts are gone, a prickling pain and feebleness remain in the legs, much like to a rheumatism ; for which, friction, with a coarse cloath or flesh-brush, is necessary. If that fails, *ung. nervinum* with *ol. terebinth.* mixed, is to be rubbed on every night, before the fire; after which, the member is to be wrapped in flannel.—If these pains are violent, or the feebleness resembles palsy; blisters ought to be applied to the skin under which the large nerves are situated, and the medicines proper in a palsy are to be given.

SOME are so wasted before the cure is completed, that they remain afterwards emaciated, weak, and as if they were hectic, with a giddiness in their head: and sometimes they chat to no purpose, or seem hypochondriac. In this condition, the patient should go to the country, to ride a-horse-back some miles every day; and, at the same time, should take bitters with bark and steel. If the giddiness continues, I have given with success, *pilul. de myrrb.* with a small proportion of *camphor.*

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E R R A T A.

Page 177. line 17. after *thing*, add *that*. p. 178. l. 16. af.
that add *which*. p. 184. l. 4. for *in* read *on*. p. 311. l. 8.
 for *falls* r. *fall*. p. 410. l. penult. f. IV. r. V. p. 413. l. 21.
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